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FISCAL AMENITIES, SCHOOL FINANCE REFORM AND THE SUPPLY SIDE OF THE TIEBOUT MARKET

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Abstract

This study asks if local governments which provide a high level of public services per tax dollar attract housing capital. The first portion of the paper examines large shifts in property tax burdens induced by an unusual school finance reform in the state of New Hampshire. The estimates suggest that, in most of the state, communities with a reduced tax burden experience a large increase in residential construction. the area of the state near the region's primary urban center (Boston), however, the shock clears through a price adjustment – i.e. by capitalizing into property values. The differing responses are attributed to differing housing supply elasticities. Furthermore, the shock induced communities with a lowered tax burden to enact more stringent land use regulations. The second portion of the paper uses a national sample and exploits variation in education spending levels arising from 1980s era school finance reforms. The results confirm the findings from New Hampshire – fiscal amenities have a significant impact on the location of residential capital and the impact is largest outside of dense, urban areas. These results, which are interpreted through the lens of a simple theoretical model, have important implications for a host of issues, including the equity and efficiency of local public goods provision, assessing who bears the burden of local taxation, and land use issues such as the location and pace of residential development and the causes of land use regulation.

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1 Introduction

Do local governments that provide more public goods per tax dollar attract more residential capital investment than other governments? Individuals choose their optimal bundle of public services through their decision on where to live (Tiebout 1956) and governments which provide public goods at a lower tax cost would be expected to experience elevated housing demand. Although a voluminous empirical literature examines the influence of public goods on the housing market, it has typically been assumed that the stock of housing is fixed.¹ With supply fixed, the housing market clears shocks to local amenities exclusively through price adjustments (i.e. through capitalization). If housing supply is elastic, however – and a developing literature suggests it is (Glaeser and Gyourko 2002, Glaeser, Gyourko, Saks 2006, Saiz 2008, and Saks 2008) – a shock to a community's fiscal amenities will induce both a price and a quantity response. This paper, therefore, examines the response of residential construction to differences in fiscal amenities across communities. Stated somewhat more broadly, the paper explores the interaction of the Tiebout market for local public goods and the supply side of the housing market. As discussed below, this topic has implications for a host of important issues, including the equity and efficiency of local public goods provision, the determination of who bears the burden of local taxation, and land use issues such as the location and pace of residential development and the determinants of land use regulation.

The empirical challenge in assessing the connection between residential investment and fiscal amenities is the endogeneity of such amenities to determinants of investment. As an example, consider tax burdens – a fiscal disamenity. Tax burdens may be high in jurisdictions which provide positive amenities such as good schools. Capital will reallocate geographically in response to tax differentials only if the differentials are not associated with differences in the level of services provided (Nechyba 2000). This paper overcomes the econometric identification problem using two distinct analyses: First, it conducts a detailed study of the housing market response to an unusual school finance reform in the state of New Hampshire. This analysis is the primary focus

¹The literature starts with Oates (1969). Recent studies examine the capitalization of education spending (e.g. Black 1999, Barrow and Rouse 2004, Hoxby and Kuziemko 2004) and property tax rates (e.g. Palmon and Smith 1998).

²Many of the studies which examine the influence of public goods on the housing market rely on the model developed by Brueckner (1979, 1982, 1983), which explicitly assumes a fixed supply of housing. Three recent papers have, however, relaxed this assumption. Hilber and Mayer (2009) use the amount of developable land as a measure of the elasticity of housing supply in a community and document that public school spending capitalizes at a higher rate in communities with less developable land. They also document that the supply of developable land influences the impact of school spending on building activity. Johnson and Walsh (2009) show that property taxes influence the location of vacation homes in Michigan. In contrast, Hoyt, Coomes and Biehl (2009) find that although property tax limitations capitalize, they do not influence housing supply.

of the paper. Second, it conducts a broader, though less detailed, examination of the housing supply response to state-level school finance reforms during the 1980s using a national sample. The national analysis draws heavily on the work of Barrow and Rouse (2004). Both analyses are interpreted through the lens of a simple theoretical model that, within the context of a growing region with multiple jurisdictions, considers both the demand for new housing and associated public good bundles and the supply of developable land.

The empirical estimates suggest that the supply of new homes is quite sensitive to fiscal amenities. The New Hampshire analysis exploits the fact that in 1999 the state began issuing extremely large lump-sum grants to municipalities as part of a school finance reform. Eighty to one hundred percent of the grants were used to fund property tax reduction and the remainder funded increased education spending (Lutz 2009). As a result, the New Hampshire analysis focuses on a particular fiscal (dis)amenity – property tax burdens. The estimates suggest that a community receiving the mean grant, equal to 15 percent of local property tax revenue, experiences an 11 percent to 22 percent increase in residential investment, implying an elasticity of roughly (negative) one. The analysis also reveals significant heterogeneity in the response. There is no evidence of an increase in investment in the New Hampshire communities within fifty miles of the nearest large city, Boston, whereas the response is most intense just outside this ring, at the urban periphery. Mirroring this result, although there is no evidence for a capitalization effect in the state as a whole, there is strong evidence of capitalization within the suburban ring. The differing responses are interpreted as reflecting differing housing supply elasticities in the suburban ring relative to the rest of the state.³ The national sample analysis focuses on a different type of fiscal amenity, education spending, as the grants provided by school finance reforms during the 1980s were used primarily to increase education funding (Barrow and Rouse 2004). The results support the two primary conclusions of the New Hampshire analysis: Residential capital investment is sensitive to fiscal amenities in many locations but not in dense, urban areas where the housing supply is less elastic.

These results have a number of important implications. First, the canonical Tiebout (1956) theory predicts that individuals will "vote with their feet" to select their preferred bundle of local public goods. Although the Tiebout theory has been the subject of a vast amount of empirical research, this first order contention of the original theory has rarely been tested.⁴ This paper can

³The reason for the variation in the housing supply elasticity is explored using data on land development density and land use regulations.

⁴Most tests of the Tiebout theory have been indirect – e.g. testing for the extent to which fiscal amenities capitalize, assessing the link between income stratification and public goods provision, testing the theory's predictions concerning community heterogeneity, etc. (see Oates 2005 and Banzhaf and Walsh 2008). Exceptions are Banzhaf and Walsh (2008) and Reschovsky (1979).

be interpreted as confirming that household location decisions are, as predicted, heavily influenced by fiscal amenities. Relatedly, it appears that Tiebout considerations influence both the physical evolution of existing communities and the pace at which this development occurs. Fiscal amenities play a particularly significant role in the formation of new communities on the urban periphery. Furthermore, it appears that fiscal amenities directly influence land use policies: New Hampshire communities which receive large grants, and hence experience a spike in building activity, are induced to increase the stringency of their land use policies. This result suggests that demand shocks cause regulation and adds to our understanding of the determinants of land use regulation, an area where the empirical evidence is thin (Saks 2008).

Second, the analysis has important implications for the cost-benefit analysis of a host of government policies. The results suggest that any policy, such as school finance reform, that de-links expenditures and taxes at the local level will cause housing capital to reallocate. This distortion needs to be included in the cost-benefit analyses of school finance reform and other, similar policies. In particular, any assessment of property taxation should take this distortion into account. The response of residential investment to property taxation has received very little empirical attention relative to the importance of the tax in the U.S. system of fiscal federalism.⁵ The estimates presented here suggest that property taxes distort the location of housing capital.

This distortion speaks to the long running debate on the incidence of the property tax – i.e. the debate over who bears the burden of the tax. There are two views. The 'benefit view' extends the Tiebout model by adding zoning regulations and capitalization of average tax price differences. Capitalization and zoning prevent individuals from free riding on public expenditures, ensure that the property tax is a payment for the use of local public services, and promote the efficient provision of public goods.⁶ Alternatively, under the 'new view', the property tax is a distortionary tax on capital and both reduces the total amount of capital in the economy and causes capital to flee high-tax jurisdictions.⁷ Determining the relative validity of the two views is important as they have very different implications for the incidence of the property tax and for assessing the efficiency of local public goods provision (Nechyba 2000, Oates 2000, Oates 1994). Existing empirical work,

⁵The property tax accounts for seventy-five percent of local taxes and is equal to three and a half percent of personal income in the U.S. (Duncombe and Yinger 2000). Wasmmer (1993) assesses the connection between property taxation and residential housing capital intensity, and Ladd and Bradbury (1988) examine the link between the property base, which includes both residential and business capital, and property tax rates. Both papers find a negative association between the capital stock and property taxes.

⁶Hamilton (1975) and Fischel (1975) originated the benefit view. Fischel (2001b) provides a recent discussion.

⁷The new view was proposed by Mieszkowski (1972). Zodrow (2000) contains a recent discussion of the different variants of the new view.

however, largely fails at this task (Nechyba 2000). Although the work here cannot be viewed strictly as a test between the theories – the type of redistributive policies examined simply rule out the benefit view – they do test, and confirm, the prediction of the new view that property tax differences not associated with differences in service levels will cause capital to flee high tax jurisdictions.

Third, the paper contributes to the emerging literature on housing supply elasticity by using a credibly exogenous housing demand shock to examine the within-MSA elasticity of supply. Most recent papers have examined the across-MSA elasticity of supply (e.g. Glaeser, Gyourko and Saks 2006, Gyourko and Saiz 2006, Gyourko 2008, and Saiz 2008). The analysis suggests that the housing supply response to a region-wide demand shock is intensely concentrated at the urban fringe – far more concentrated than the geographic distribution of overall building activity.

Finally, the paper highlights the danger of interpreting house price capitalization estimates as the marginal willingness to pay for local amenities, a widely used practice in empirical research. Such an interpretation becomes problematic if the quantity side of the housing market is responsive to amenity shocks because capitalization estimates will then underestimate the marginal willingness to pay (Glaeser, Gyourko and Saks 2006). The analysis provides not only evidence of the potential severity of this problem, it also provides a starting point for gaining traction on the problem – capitalization estimates from relatively dense urban and suburban areas are more likely to reflect the true marginal willingness to pay for an amenity than estimates from the urban fringe or beyond.

The paper proceeds as follows. Section II presents a simple theoretical model of the how the housing market clears fiscal shocks. Section III presents the New Hampshire analysis. Section IV presents the national analysis. Section V discusses policy implications and Section VI concludes.

2 A Simple Model of Housing and Fiscal Amenities

This section presents a model of how the housing market clears fiscal shocks. The model, which integrates a local public goods model with a model of land development timing under perfect foresight, is narrowly focused on providing a framework for interpreting the empirical results.⁹

⁸Recent examples of non-fiscal capitalization studies include Bui and Mayer (2003), Davis (2004), Chay and Greenstone (2005), Greenstone and Gallagher (2008) and Linden and Rockoff (2008).

⁹Ross and Yinger (1999) provide a general treatment of the interaction of local public finance and the housing market and review the literature. The literature on the timing of real estate investment and urban growth includes. among others, Bentick (1979), Bentick and Pouge (1988), Cappoza and Helsley (1989, 1990) and Capozza and Li (1994).

2.1 Assumptions

In each time period t, a fixed number of individuals arrive in a metro area and must choose between residing in community A or any one of a large number of identical alternative communities, referred to collectively as community B. Once a community has been chosen, the individual resides there in perpetuity. Assume initially that the number of housing units is fixed in A. Community A is small relative to the surrounding communities so the rental rate for housing in community A, $r_{h,a}$, does not affect the rate in Community B, $\bar{r}_{h,b}$, which is considered fixed.

The communities provide a single public good, education, funded by a local property tax and grants from the state. The community budget constraint is therefore:

$$e_m = g_m + T_m \tag{1}$$

where e_m is per-household education provision in municipality m, g_m is the per-household grant and T_m is the per-household property tax (a time subscript t is suppressed for now). The communities have pre-existing residents who have, exogenous to this model, determined the level of local public goods and the tax burden (see Lutz 2009 for a model of this process relevant to the New Hampshire communities examined empirically in this paper).

Individual i chooses a community of residence so as to maximize utility (quasi-linear in form) subject to a budget constraint.

$$U_i(x, e_m, m) = \Phi(e_m) + \Psi_i(m) + x \tag{2}$$

$$y = x + r_{h,m}\bar{h} + T_m \tag{3}$$

where x is a numeraire consumption good with a price normalized to 1 and y is income. $\Psi_i(m)$ is the individual specific premium to residing in community B arising from non-fiscal amenities: $\Psi_i(m) = \{ \substack{0 \text{ if } m=A \\ \pi*i \text{ if } m=B }; i \text{ indexes individuals with respect to their non-fiscal preference for residing in community } B$ and π quantifies how compact or disperse these benefits are among the population of homebuyers (the π parameter is discussed more fully below). All homebuyers purchase a single unit of identical housing and land, $\bar{h} = 1$ (implying an equal property tax burden for all individuals within a given community). Housing does not directly enter the utility function – purchasing a house is equivalent to purchasing the local public good provided by a municipality.

Equilibrium in the housing market requires that the marginal homebuyer, i^* , is indifferent between community A and B

$$\Phi(e_a) + y - r_{h,a}\bar{h} - T_a = \Phi(e_b) + \pi * i^* + y - \bar{r}_{h,b}\bar{h} - T_b$$
(4)

where * denotes the marginal homebuyer. Define δ_{i^*} as the difference in rents between the communities which satisfies equation (4)

$$\delta_{i^*} = \bar{r}_{h,b} - r_{h,a} = \Phi(e_b) - \Phi(e_a) + T_a - T_b + \pi * i^* = S_b - S_a + \pi * i^*$$
(5)

where S_m is the fiscal surplus of community m – the difference between the benefit received from the local public good and the tax paid: $S_m = \Phi(e_m) - T_m$. The rent differential, δ_{i^*} , is equal to the difference in the fiscal surplus in the two communities plus the value to the marginal homebuyer of the non-fiscal amenities in community B.

House values, p_m , are equal to the discounted stream of rental payments, $\frac{r_{h,m}}{k}$, where k is the discount rate. $r_{h,a}$ is equal to the rental payment in B minus δ_{i^*} , the rent deferential required to make the marginal homebuyer indifferent between A and $B: p_a = \frac{r_{h,a}}{k} = \frac{\bar{r}_{h,b} - \delta_{i^*}}{k}$.

Panel A of Figure 1 graphically displays equilibrium in the market for new housing in community A. The downward slopping line, which can be viewed as an aggregate demand curve, is the price of housing at which each individual i would be indifferent between residing in community A or B. The fixed supply of new housing in community A determines the identity of the marginal homebuyer. If there are q units of available housing, then individual i = q is the marginal homebuyer and all individuals i > q reside in community B. The market price of housing is set at the point at which the marginal homebuyer i^* is indifferent between the two communities.

2.2 Fiscal Shock with Fixed Housing Supply

An increase in the per-pupil grant received by community A will change the equilibrium price of housing

$$\frac{\partial p_a}{\partial g_a} = -\frac{\partial \delta_{i^*}}{\partial g_a} * \frac{1}{k} = \left[\frac{\partial \Phi(e_a)}{\partial e_a} * \frac{\partial e_a}{\partial g_a} - \frac{\partial T_a}{\partial g_a}\right] * \frac{1}{k}$$
 (6)

Define the marginal propensity to spend out of grant income of community A's decisive voter as α : $\frac{\partial e_a}{\partial g_a} = \alpha$. Total differentiation of the community budget constraint, equation (1), yields $\frac{\partial T_a}{\partial g_a} = \alpha - 1$. Equation (6) can now be re-written as

$$\frac{\partial p_a}{\partial q_a} = \left[\frac{\partial \Phi(e_a)}{\partial e_a} * \alpha - (\alpha - 1)\right] * \frac{1}{k} \tag{7}$$

Full capitalization occurs when the increase in house prices equals the discounted stream of grant payments: $\frac{\partial p_a}{\partial g_a} = \frac{1}{k}$. Thus, the grants will fully capitalize if the marginal propensity to spend on education is 0 ($\alpha = 0$) or if the marginal homebuyer values additional education spending at its cost or more ($\frac{\partial \Phi_i(e_a)}{\partial e_a} \ge 1$). If both of these conditions fail to hold, the grants will capitalize at less

than their full value and the extent of capitalization will be a function of α and $\frac{\partial \Phi_i(e_a)}{\partial e_a}$.

The change in property values can also be derived in terms of fiscal surplus. The change in fiscal surplus induced by a change in grants is $\frac{\partial S_a}{\partial g_a} = \frac{\partial \Phi(e_a)}{\partial e_a} * \alpha - (\alpha - 1)$. Substituting the expression into equation (7): $\frac{\partial p_a}{\partial g_a} = \frac{\partial S_a}{\partial g_a} * \frac{1}{k}$. This result is displayed graphically in panel A of Figure 1 as an outward shift in the aggregate demand curve for new housing in community A equal to $\frac{\Delta S_a}{k}$. The fixed supply of housing units means the market clears the shock to the fiscal surplus solely through a price response. The identity of the marginal homebuyer is unchanged.

2.3 Fiscal Shock with Elastic Housing Supply

The above analysis assumes that the supply of new housing is fixed in community A. Several arguments have been made to justify this assumption. Community boundaries are fixed (Epple and Romer 1989) and housing supply cannot be elastic in a jurisdiction with fixed boundaries (Ross and Yinger 1999). While this argument may be true in communities which are fully developed, it is not necessarily true if developable land exists. Land will be bid away from an alternative use, such as agriculture, and into residential use when residential rents exceed agricultural rents (Capozza and Helsley 1989). A recent example of housing supply elasticity arising from land conversion is provided by northern California. In recent years timber companies rapidly sold off land that had become more valuable for residential development than for logging (Eilperin 2006). Similarly, a shock to fiscal amenities which raises residential rents will provide an incentive to shift land into residential use (Hamilton 1976a). Zoning is another potential source of inelasticity. However, Glaeser and Gyourko (2002) present evidence that the impact of zoning varies greatly across the U.S. and has little effect in many locations. Ultimately the elasticity of housing supply is an empirical question and a recent literature suggests that supply is elastic in many part of the country – see Saks (2008) for a review of the literature.

Assume now that the supply of new housing is determined endogenously by agricultural landowners who choose when to convert their land to residential use. As is typical in the literature on land development, assume that the optimal form of residential development changes with time and that, as a result, the new housing rental rate in community B grows exponentially¹⁰: $r_{h,b}(t) = \overline{r}_{h,b}e^{gt}$, where $\overline{r}_{h,b}$ is the rental rate in community B at time t = 0. Development is considered irreversible and residential rents are therefore permanently equal to their value at the time of development.

 $^{^{10}}$ The gradual growth in rents could be justified by modifying a few of the assumptions given at the start of section 2. For instance, it could be assumed that each successive cohort of new homebuyers has a larger income y than the previous cohort and that higher income cohorts build more capital intensive homes – i.e. larger homes per unit of land. The increase in capital intensity would cause the land rental rate to rise over time.

At time T the owner of a piece of land with agricultural rents r_{agr} and conversion costs c + z, where c captures physical conversion costs and z captures regulatory costs (e.g. zoning), chooses the conversion time, T^* , to maximize the value of the land $V_{agr,a}$

$$\max_{T^*} V_{agr,a} = \int_{T}^{T^*} r_{agr} e^{-k(t-T)} dt + \int_{T^*}^{\infty} r_{h,a}(T^*) e^{-k(t-T)} dt - (c+z) e^{-k(T^*-T)}$$
(8)

Substitute into the agricultural land value equation the marginal homebuyer's indifference relation at time T^* , $r_{h,a}(T^*) = r_{h,b}(T^*) - \delta_i = \overline{r}_{h,b}e^{gT^*} - \delta_i$, evaluate (equation (9)), and then solve for the optimal conversion time T^* (equations (10) and (11))

$$V_{agr,a} = \frac{r_{agr}(1 - e^{-k(T^*-T)}) + r_{h,b}e^{-(k-g)T^*+kT} - \delta_i e^{-k(T^*-T)}}{k} - (c+z)e^{-k(T^*-T)}$$
(9)

$$\frac{\partial V_{agr,a}}{\partial T^*} = \frac{r_{agr}e^{-k(T^*-T)}k + r_{h,b}e^{-(k-g)T^*+kT}(g-k) + k\delta_i e^{-k(T^*-T)}}{k} \tag{10}$$

$$T^* = \frac{\ln(\frac{(r_{agr} + \delta_i + (c+z)k)k}{r_{h,b}(k-g)})}{a} = \frac{\ln(\frac{(r_{agr} + S_b - S_a + \pi * i^* + (c+z)k)k}{r_{h,b}(k-g)})}{a}$$
(11)

The optimal conversion time, T^* , is reduced when fiscal surplus, S_a , increases

$$\frac{\partial T^*}{\partial S_a} = \frac{-1}{g(r_{agr} + \delta_i + (c+z)k)} < 0 \tag{12}$$

The supply of new housing in community a is thereby increased; plots of agricultural land which previously were receiving agricultural rents ε too large to convert at time t will now convert at time t.

Intuitively, the positive fiscal shock increases demand for housing in community A and thereby increases the rent such housing commands. Landowners who were previously better off continuing to farm their land at time t and convert at a later date are now induced to convert immediately because the difference in agricultural and residential rents has shifted in favor of residential use. The increased number of conversions increases the supply of new housing. In order for the market to be in equilibrium, the number of homebuyers must increase to equal the number of newly converted plots of land. Given the downward sloping demand curve for housing in community A, the increase in homebuyers reduces the extent of capitalization relative to the fixed supply case. In the fixed supply case, the grants capitalize at $\frac{\Delta S_a}{k}$, while with elastic housing supply they capitalize at $\frac{\Delta S_a - \Delta^* i * \pi}{k}$, where $\Delta^* i = i^{**} - i^*$, $i^{**} > i^*$ and i^{**} and i^* are the number of total homebuyers in the presence of the fiscal shock and in the absence of the shock, respectively. Panel B of Figure 1 displays this graphically.

The model yields predictions about how community characteristics will influence the price and quantity response to a fiscal shock. The elasticity of housing supply is generally influenced by the amount of land available for development, the stringency of zoning and the cost of construction (Saks 2008). The model captures these three factors. A fiscal shock will tend to reduce the time to conversion by a lesser amount (i.e. the quantity response is reduced) when agricultural rents are high: $\frac{\partial T^*}{\partial S_a \partial r_{agr}} = \frac{1}{g*(r_{agr} + \delta_i + (c+z)k)^2} > 0$. As a result, communities further along in the development process, which will have a relatively small supply of land remaining for development, will have a less elastic supply because the remaining land will be highly productive in agricultural use. This is displayed graphically in panel C of Figure 1, where S1 represents the community with a relatively inelastic housing supply. More stringent zoning will also tend to make housing supply less elastic: $\frac{\partial T^*}{\partial S_a \partial z} = \frac{k}{g*(r_{agr} + \delta_i + (c+z)k)^2} > 0$. Mayer and Somerville (2000) document empirically that land use regulation reduces housing supply elasticity. Finally, higher construction costs will reduce supply elasticity: $\frac{\partial T^*}{\partial S_a \partial c} = \frac{k}{g*(r_{agr} + \delta_i + (c+z)k)^2} > 0$.

On the demand side of the market, the response to the reform will be a function of the net benefits to living in community B arising from non-fiscal amenities, $\Psi_i(m)$. These benefits range over $[0, \pi * I]$, where I is the total number of homebuyers i. The π parameter determines how compact or diffuse the benefits are among the population of homebuyers. The smaller π is, the more compact the benefits. The more compact the benefits, the greater the quantity response: $\frac{\partial T^*}{\partial S_a \partial \pi} = \frac{1}{g(r_{agr} + \delta_i + (c + z)k)} > 0 \text{ (i.e. with compact benefits, the negative influence of a shock on conversion times is intensified). Intuitively, compact benefits can be understood as indicating that community <math>A$ and B are close substitutes. When communities are close substitutes, a large number of homebuyers will be induced to switch communities in response to a fiscal shock. If communities are not close substitutes, fewer homebuyers will be induced to switch and the price and quantity response will be muted. This is displayed graphically in panel D of Figure 1. Demand curve D2 arises from a smaller π than D1, $\pi_1 > \pi_2$. The more compact distribution of benefits generated by π_2 implies a relatively less steeply sloped demand curve.

Although the empirical examples considered in this paper focus on shifts in taxation and spending, the model's predictions are applicable to any fiscal policy which generates differentials in fiscal surplus between communities. Such differentials can arise for many reasons, including differences in the efficiency of public goods provisions and differences in the mix of public goods.

¹¹The addition of uncertainty into the model would work to intensify this effect by introducing option value considerations (Noxy-Marx 2007, Capozza and Li 1994 and Capozza and Helsely 1990).

3 New Hampshire Analysis

The New Hampshire analysis focuses on property taxes. A 1999 reform provides unusually credible exogenous variation in property tax burdens.

3.1 Background Information

Prior to 1999, New Hampshire education was funded primarily by local property taxes. Eighty-seven percent of total primary and secondary education revenue came from the local level — the highest in the nation. The state with the next highest percent, Connecticut, attributed 57 percent of total revenues to local sources and the median state, Wisconsin, attributed 41 percent.¹² Nine percent of education revenue was provided by the state and the remainder was provided by Federal funding.

The reliance on local, property tax based financing created significant dispersion in per-pupil funding and property tax burdens across municipalities. In the *Claremont II* ruling, the New Hampshire Supreme Court declared the local property tax used to fund K–12 education unconstitutional. The ruling found the existing school finance scheme provided inadequate educational opportunity in property-poor towns and imposed inequitable tax burdens.

In response to the *Claremont* ruling, the state legislature enacted a major reform in November of 1999. Under the reform, eighty percent of communities receive positive lump-sum grants from the state. The remaining communities, referred to as 'donor towns', are forced to remit payments to the state (i.e. they receive negative grants) The primary determinant of a municipality's grant is per-pupil property wealth – the lower a town's per-pupil property wealth, the larger the grant.

The payments of the donor towns funded only a small portion of the total cost of the reform. The remaining revenue was raised by increasing several state-wide taxes and the use of lottery revenue. None of the taxes increased were property based, nor is there any obvious reason why the incidence of these taxes would differ by municipality or geographic region.

The reform was large in magnitude: The new funding provided, \$276 million, is equal to 19% of total pre-reform education revenue in the state. In addition, another \$130 million, primarily representing funds from former programs cancelled as part of the reform, was subject to redistribution. Figure 2 displays the large shift from local to state financing produced by the reform.¹³

Economic theory predicts that a lump-sum grant to a locality will be spent on public goods at

¹²U.S. Bureau of the Census (2000)

¹³The treatment of the 1999 reform is a simplification which highlights the important elements. See Lutz (2009), and the references it contains, for more detailed information.

the communities marginal propensity to spend on public goods out of private income (Bradford and Oates 1971 a,b). As the marginal propensity to spend on public goods is estimated to be between 5 to 10 cents on the dollar, theory predicts that only 5 to 10 cents per dollar of grant income will be used for increased public goods provision, including education. The rest will be spent on private consumption. In New Hampshire, where virtually all own source revenue is derived from the property tax, this would occur via a reduction in the rate of property taxation. A large empirical literature, though, contradicts this prediction and finds that grants are systematically spent as intended by the sending government. This empirical tendency has been termed the "flypaper effect" because it documents that grants tend to stick where they are targeted (Hines and Thaler 1995). The empirical tendency creates the expectation that only a limited portion of the New Hampshire grants will be spent on property tax reduction.

Despite this expectation, Lutz (2009) documents that the New Hampshire grants were subject to little to no flypaper effect. Estimates of the portion of the grants used to fund property tax reduction range from 80 to 100 hundred cents per grant dollar (a preferred set of specifications produce estimates of around 90 cents). The lack of a flypaper effect may be attributable to New Hampshire's use of a form of direct democracy for determining the annual provision level of local public goods. The system, which involves citizens voting directly on budget items in a town meeting format, likely expresses the decisive voters preferred level of spending. In contrast, most studies which document a flypaper effect do so in environments in which it is less clear whose preference are determining budgeting decisions.

The reform is almost certainly permanent because it is based on a ruling by the state Supreme Court and can only be revoked by an amendment to the state Constitution. Such an amendment was attempted and failed by a substantial margin. The long run nature of the reform is important because capital investment decisions are made on the basis of expected long run tax burdens. If the reform was short run in nature, capital investment would be less likely to respond. Similarly, the extent of capitalization would be reduced if the reform was viewed as impermanent (Ross and Yinger 1999; Yinger, Borsch-Supan, Bloom and Ladd 1988).

Finally, in relation to the predictions of the model, two points about the New Hampshire environment deserve emphasis. First, the model only predicts that residential investment will reallocate within New Hampshire in response to the reform. The grants are funded with increased statewide taxes. On average, the total tax burden faced by a resident of the state is not altered and there is therefore no expectation that the total amount of investment in the state will change.

Second, although developed land and structures are taxed at their market value in Hew Hampshire, undeveloped land is not. A hypothetical 50 acre tract in agricultural use would face an annual tax burden of around \$300. The same tract, in use for residential purposes, would face a \$7,000 tax bill exclusive of the tax on housing capital (Ruedig and Gartrell 2002). The different tax treatments are important because if undeveloped and developed land face the same tax burden, a negative shock to the burden would not be expected to cause a housing supply response. In terms of the model, a property tax reduction would cause the supply curve in panel B of Figure 1 to shift upward by the same amount as the demand curve (because after-tax returns to agriculture would increase). The price of new housing would increase (from P to P'), but the quantity would remain unchanged. The lower tax on agricultural land in New Hampshire reflects typical practice in the U.S.: Land that is vacant or in use for agricultural purposes is almost always taxed at a fraction, typically a small fraction, of the land's market value (Vitaliano and Gravelle 2005). 14

3.2 Data and Summary Statistics

The data used in the New Hampshire analysis come from multiple sources. Building permit data for new single family homes, collected by the U.S. Census Bureau, measures investment in residential capital.¹⁵ Sales price data, collected by New Hampshire Housing Finance Authority, measures property values. Property tax data come from the New Hampshire Department of Revenue Administration and the reform grant data come from the New Hampshire Departments of Education and Revenue Administration. Data on land use regulation come from the New Hampshire Office of Energy and Planning and a dataset compiled by Richard England. The Office of Energy and Planning data are supplemented by a survey of municipalities conducted by the author. Data on land use are from Hilber and Mayer (2009). Finally, demographic data are from the 2000 Census. See the Data Appendix for additional information.

Table 1 displays municipality means in 1998 (the year prior to the reform), 2000 and 2002. The first row displays the measure of the fiscal shock induced by the reform, $\frac{netgrant_{m,99}}{ptax_{m,98}}$, where $netgrant_{m,99}$ is municipality m's net grant in 1999, the first year of the reform, and $ptax_{m,98}$ is total property tax payments in 1998, the year prior to the reform (both expressed in constant 1999 dollars). The net grant is equal to a municipality's reform grant minus funds lost as part of the reform. The fiscal shock measure is easily interpreted. It is the percent reduction in each property

¹⁴The lower tax burden on agricultural and vacant land is often motivated by conservation concerns, as well as a desire to preserve family farms. The lower tax burden is achieved in different ways across the states, although assessing the land at its worth in "current-use", as opposed to its highest value use (i.e. market value), is by far the most common (Vitaliano and Gravelle 2005). In many states the lower tax burden is voluntary and is associated with a penalty at the time of development (England, 2002).

¹⁵Although a measure of the dollar value of residential investment would be useful, an accurate measure of this type is not avaliable.

owner's tax burden assuming all grant funds are used for tax reduction. The mean fiscal shock is equal to 0.15 (row #1), indicating the mean municipality would have been able to achieve a 15% reduction in its tax burden. The 10th percentile municipality experiences a negative shock of -0.05 (row #2). This community, which has high per-pupil property wealth, receives no grant and is forced to make an excess tax payment to the state. The shock at the 90th percentile is 0.29 (row #4). This low per-pupil property wealth community receives aid equal to almost a third of total local tax revenue. An alternative measure of the fiscal shock is $\frac{netgrant_{m,99}}{taxbase_{m,98}}$, where $taxbase_{m,98}$ is the property tax base in the year prior to the reform (row #5). The alternative measure can be interpreted as the reduction in the property tax rate induced by the reform.

Conditional on receiving a positive net grant, both the municipal tax rate and total tax burden declined between 1998 and 2000 (rows # 6 and #8). Conditional on receiving a negative net grant, both the tax rate and total tax burden increased (rows # 7 and #9). Tax burdens increased for both types of towns after 2000, primarily reflecting increased education spending. (Education spending also rose in neighboring New England states over this period.) Despite the increased tax burden, tax rates fell substantially over this same period as the result of rapidly increasing property values.

Panel A of Figure 3 displays the mean values of two of the outcome variables used in this study: residential investment and house prices. Residential investment is measured by $\frac{permits_{m,t}}{hstock_m}$, where $permits_{m,t}$ is the number of single family home building permits at time t and $hstock_m$ is the stock of existing single family homes as measured in the first year of the sample, 1996. House prices are measured as the mean sales value of existing homes in a municipality. The figure suggests that demand for housing in New Hampshire was increasing over this period – both prices and investment (i.e. the quantity flow) rose steadily.

3.3 Empirical Model and Identification

The most general empirical specification would estimate the effect of fiscal surplus, $S_{m,t}$, on residential investment

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta S_{m,t} + \phi_t + \eta_m + \varepsilon_{mt}$$
(13)

Equation (13), however, suffers from clear endogeneity bias – fiscal surplus is almost certainly correlated with determinants of investment – and an instrumental variables (IV) strategy would be

¹⁶See the Data Appendix for additional information. The New Hampshire empirical work uses single family home building permits as the metric for residential investment. The results are robust to using total housing unit building permits.

required. The reform grants are an obvious choice for instruments. The IV strategy is infeasible, though, because municipal fiscal surplus is unobservable. The following empirical model, which can be viewed as a reduced form version of the IV strategy, is used instead

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \phi_t + \eta_m + \varepsilon_{mt}$$
(14)

where $postreform_t$ is an indicator variable equaling one in years greater than or equal to 1999, the first year of the reform.

Although the reform grants can be viewed as an exogenous shock to a municipality's budget constraint, they may still be correlated with ε_{mt} for several reasons. First, the reform grants were recalculated annually after the second year of the reform. The recalculated reform grants may reflect adjustment to the reform. Of particular concern, they may reflect residential investment endogenous to the reform – i.e. increased investment in response to the reform will increase aggregate property values and, all else equal, reduce the size of grant. The grant is therefore held fixed at its initial level. The changes in the grants from year to year were small and the initial grant level can be considered a proxy for the grants received over the 2000 to 2004 period. Second, the fiscal shock measure is a function of the time-invariant arguments of the grant formula (primarily per-pupil property wealth) and the municipal tax burden in the year prior to the reform. Municipal fixed-effects, η_m , control for possible correlation between these variables and unobserved determinants of capital investment.

The models are estimated with data ranging from 1996 to 2003, with 1999 omitted from the sample. 1999 is omitted for two reasons. First, the reform was announced in November of 1999 and it is unlikely that there was a significant investment response in the remainder of the year. Second, when the reform was announced in late 1999, municipal budgeting decisions for the year had already been made. Many municipalities were constrained from reacting to the grants by the late announcement and it may have been unclear how a given municipality would respond in the long-run (i.e. if the grants would be used for tax reduction or increased government spending). In 2000 municipalities were unconstrained. Investment decisions are based on the long-run expected tax burden of a community, not the burden arising in a single year due to short term constraints. The

¹⁷An alternative approach would be to estimate $\frac{permits_{m,t}}{hstock_m} = \alpha + \beta_1 \frac{netgrant_{mt}}{ptax_{mt}} + \phi_t + \eta_m + \varepsilon_{mt}$ and instrument the time-varying fiscal shock measure with $\frac{netgrant_{m,99}}{ptax_{m,98}}$ interacted with a vector of year dummies for the post reform years 2000 - 2004 (the interactions are needed because the upward trend in the tax base, $ptax_{mt}$, causes the gradient of the instrument with respect to the time-varying shock measure to shift over time). The results produced with this specification are extremely similar to the results produced using equation (14). Equation (14) is preferred over the instrumenting procedure because of its greater transparency. This is particularly valuable in the estimates which utilize the fiscal shock measure interacted with other variables (e.g. Tables 6 and 7).

estimates presented in the paper are not substantively changed if 1999 is included. Municipalities are excluded from the sample if they fail to have non-missing data for at least six of the seven years. Finally, very small municipalities, those with less than 1200 residents in year 2000, are dropped from the sample.

3.4 Results

3.4.1 The Quantity Response: The Response of Residential Investment to the Fiscal Shock

Table 2, column (1), presents the results of estimating equation (14). The β estimate is precise and economically large. Evaluated at the mean value of the fiscal shock measure, the estimate implies a 0.17 increase in the rate of residential investment (note that the dependent variable has been multiplied by 100 for presentation reasons). Using the mean rate of investment in 1998, 1.6, this implies the fiscal shock induced an 11 percent increase in the rate of residential investment. The mean value of the fiscal shock measure is 0.15, interpretable as a 15 percent decrease in the property tax burden. The estimate can therefore be interpreted as implying that the elasticity of residential construction with respect to the property tax burden is very roughly equal to (negative) one. The results can also be understood relative to the municipal capital stock, measured as the number of single family housing units in the first year of the sample. The estimates imply that ten years after the initiation of the reform, the capital stock will have increased by around 2 percent in a town with the mean fiscal shock.

After the announcement of the reform, it may have taken time for investment to fully respond because construction takes time to implement. Column (2) explores this possibility by allowing the coefficient on the fiscal shock measure to vary by year. The results indicate that the response to the reform was immediate and displays no trend over time. Column (3) employs the alternative fiscal shock measure, $\frac{netgrant_{m,99}}{taxbase_{m,98}}$, and produces results similar to the specifications using the standard measure.

In columns (1) - (3) the dependent variable is defined as the ratio of building permits in a year to the stock of homes at the start of the sample period. This construction is useful because it allows the estimation of a proportional response – i.e. the marginal effect of the grants on building permits is estimated proportionally to the stock of housing. Although the ratio of permits to the housing stock is easily interpreted and is quite intuitive – indeed, it is the measure that many municipalities use when officially quantifying their rate of growth in land use regulations – it could be argued that the normalization is arbitrary. Column (4) presents estimates from a model using the log of building permits as the dependent variable. The specification must be altered because roughly

2 percent of the building permit observations are equal to zero. The sample is first restricted to towns with a year 2000 population exceeding 1,600 and non-missing building permit data for all years of the sample. The data is then collapsed into a single pre-reform and a single post-reform observation for each municipality by summing over the pre and post periods. The new dataset contains non-zero building permit counts for all observations. Compared to the sample used in columns (1) - (5), the new sample contains approximately 85 percent of the municipalities and 95 percent of the building permits. The estimate in column (6) suggests that the typical community experienced a 15 percent increase in the pace of building activity – similar to the estimate in column (1).

In order for the β estimates to be interpreted as capturing the causal impact of the reform on investment, it must be assumed that the fiscal shock-post-reform interaction, $\frac{netgrant_{m,99}}{ptax_{m,98}} * postre form_t$, is uncorrelated with time-varying determinants of investment activity. The surge in housing demand over this period makes scrutinizing this assumption particularly important. If the intensity of the demand surge across communities was associated with the level of per-pupil property wealth, the estimates are likely to be biased. It is possible, for instance, that demand for communities with elevated levels of per-pupil property wealth – e.g. resort communities – rose at a particularly fast clip. Such a scenario would produce downward bias in β . A two part approach is taken to address concerns of this type. First, three additional models are estimated as robustness checks. These models attempt to control for time-variant determinants of investment. Second, a falsification test is executed.

The first of the robustness checks is

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \varphi_t * X_m + \phi_t + \eta_m + \varepsilon_{mt}$$
 (15)

where X_m is a vector of municipal characteristics measured in the first year of the sample, 1996, and φ_t is a vector of time-varying coefficients.¹⁸ The model controls for changes over time in investment that are associated with these fixed characteristics. For example, distance from Boston controls for rapid growth in southern New Hampshire, a density measure controls for the less elastic supply of housing in more dense communities, and the percent of property for recreation use controls for time-varying demand for second homes.¹⁹ The second robustness check controls

¹⁸None of the models include time-varying variables, such as demographic characteristics, as controls because changes in such variables may be endogenous to the reform.

¹⁹The complete set of characteristics in the X_m vector are distance from Boston, distance from Boston squared, municipal population, municipal population squared, the percent of municipal property that is residential, the percent of municipal residential property that is for seasonal or recreation use, and municipal density (defined as the total

for municipal-specific trends in construction by adding linear trend terms, $\eta_m * t$, into the model.

Column (5) adds the base year characteristic interaction terms, equation (15), and column (6) adds the linear trend terms. The inclusion of the base-year characteristics almost doubles the magnitude of the estimate (relative to column (1)). The grants are estimated to increase the investment rate by 22 percent. Inclusion of the linear trend terms, however, both diminishes the size of the point estimate and reduces its precision. As is always a concern with specifications of this type, it is possible that the trend terms are absorbing some of the impact of the event being studied – the grant introduction – and that the point estimate is therefore biased downward. Estimates discussed below provide support for this hypothesis.

The final column of Table 2 presents the falsification check, enacted as follows: A "placebo" fiscal shock is generated by assigning each New Hampshire municipality in 1998 the shock it actually received in 1999

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \beta_{place}o \frac{netgrant_{m,99}}{ptax_{m,98}} * prereform_t + \phi_t + \eta_m + \varepsilon_{mt}$$

$$(16)$$

where $prereform_t$ equals one in 1998. 1996 and 1997 are the omitted year categories for the vector of fiscal shock-time period interaction terms. The rationale behind the test is straightforward – there should be no response to the fiscal shock in the year prior to the reform. The results of the test are encouraging. The placebo point estimate is negligible and the true fiscal shock coefficient is little changed. Thus, the falsification test provides no evidence against a causal interpretation of the estimates in columns (1) - (6).

3.4.2 New England States Robustness Check

The final robustness check would ideally be executed as

$$\frac{permits_{m,t}}{hstock_m} = \alpha + \beta \frac{netgrant_{m,99}}{ptax_{m,98}} * postreform_t + \beta_d determinants_m * postreform_t + \phi_t + \eta_m + \varepsilon_{mt}$$

$$(17)$$

where $determinants_m$ is the vector of arguments appearing in the grant formula. The specification controls, in a time-varying manner, for the determinants of the magnitude of the fiscal shock. Thus, it addresses in a relatively direct fashion the concern that the β estimates are biased because the fiscal shock is correlated with time-varying determinants of investment activity. Although equation (17) is not viable when estimated using data only from New Hampshire – the grants are

number of housing units divided by land area). The percent of homes used for seasonal or recreational use and the percent of the tax base that is residential are only available as measured in 2000.

primarily a linear function of $determinants_m$ – it becomes viable with the inclusion of data from other states. Surrounding New England states provide 'control' municipalities and the fiscal shock variable is set equal to zero for these communities. The approach is similar in spirit to a triple difference-in-difference estimator with the identifying variation coming from the interaction of three variables: the fiscal shock measure, a post reform indicator and a New Hampshire indicator.

A practical problem with estimating equation (17) is the unobservability of the $determinants_m$ vector for the control states. A measure of per-child residential property wealth, taken from the 2000 Census, is therefore used as a proxy for the $determinants_m$ vector. The census measure is a strong predictor of the fiscal shock: The two variables have a cross-sectional correlation of -0.76 in the New Hampshire sample.

The specification rests on the assumption that the other New England communities are a valid counterfactual for the New Hampshire communities. Although this assumption is inherently untestable, the control groups are constructed to be similar to New Hampshire along observable dimensions. Column (1) of Table 3 presents demographic characteristics for the New Hampshire sample. Column (2) presents the demographics for the 'Southern Maine' control group and Figure 4 maps its geographic boundaries (the precise geographic definitions of the control groups, as well as the rationale underlying their definitions, are provided in the Data Appendix). Other than somewhat lower income median incomes and median home values, the Southern Maine communities are remarkably similar to their New Hampshire counterparts. Most importantly, the average per child residential property wealth is extremely comparable. Furthermore, as displayed on Panel B of Figure 3, the two groups display a similar upward trend in building activity over the sample period.²⁰

Table 4 presents the results of estimating the robustness check. Column (1) displays the results when no control communities are included. As expected given that per-child residential property wealth is a strong linear predictor of the fiscal shock, the β point estimate is small and imprecise. With the inclusion of the 'Southern Maine' control group in column (2), however, the estimate becomes precise and similar in magnitude to those on Table 2. Column (4) uses 'Western Massachusetts' as the control group and the estimate becomes somewhat larger, but remains within the range of those on Table 2.

The robustness check may be invalid, or contaminated, if the New Hampshire reform induced changes in investment activity in the control groups. Although the impact of the reform outside

²⁰It is important to keep in mind that, as discussed above, the only unambigious theoretical prediction is that the grants will cause investment to reallocate *within* New Hampshire. The impact on total investment in the state is unclear because, on average for the state as a whole, the statewide tax increases required to fund the grants offset their benefits.

of New Hampshire is theoretically ambiguous because of the increase in statewide taxes used to fund the grants, it is possible that the reform influenced investment decisions across state borders for certain communities. For instance, investment in property wealthy communities located near the Maine-New Hampshire border may have reallocated toward Maine because the New Hampshire comminutes received no benefit from the reform (or even received a negative shock), but were required to pay higher statewide taxes. To address this concern, column (6) uses a control group comprised of 'Southeastern Maine'. The municipalities in this control group are a minimum of 40 miles from the border with New Hampshire, have similar observables to New Hampshire and a similar trend in building activity, although the sample size is equal to only a bit less than half of the New Hampshire sample size. The results from employing this control group are again quite similar to those on Table 2. Column (8) adds a portion of 'Central New England' to increase the size of the control group and produces very similar results. Finally, using all communities in Connecticut, Massachusetts, Maine and Rhode Island as a control group – column (10) – also produces similar results. Vermont is not used to provide control communities because, as is clearly visible in Appendix Figure A1, it enacted a school finance reform in 1998.²¹ The remaining New England states had stable school finances over the period.

Table 4 also displays estimates from specifications which include municipal-specific trend terms. In contrast to Table 2, the point estimates are robust to the inclusion of the trend terms (although the Southern Maine estimate is significant only at the 10% level).

3.4.3 Heterogeneity In the Quantity Response

If there is heterogeneity in the elasticity of housing supply, there will be heterogeneity in the response of investment to the fiscal shock. As discussed in the theory section, heterogeneity in the elasticity of supply can occur for a number of reasons, including variation in the extent of land use regulation and the amount of land available for development.

²¹The 1998 Vermont school finance reform potentially has implications for the results displayed on Table 2. The reform, by altering the fiscal surplus available in Vermont municipalities, may have affected residential investment in surrounding states, including New Hampshire. To the extent that the reform increased or decreased the relative fiscal surplus of all New Hampshire municipalities, the year effects in equation (14) will control for the Vermont reform. There are circumstances under which the year effects will fail to control for the Vermont reform. For instance, property wealthy Vermont municipalities may be close substitutes for property wealthy New Hampshire municipalities. If the Vermont reform made property wealthy municipalities in Vermont less desirable by decreasing their fiscal surplus, investment may have increased in property wealthy New Hampshire municipalities. Similarly, the reform may have made property poor Vermont towns more attractive and decreased investment in property poor New Hampshire municipalities. This scenario, and most other reasonable scenarios, implies a downward bias in the estimates on Table 2. Furthermore, the available evidence suggests the Vermont reform does not bias the estimates. The placebo falsification test in column (7) of Table 2 provides no evidence that the Vermont reform biases the estimates. The placebo grant is "turned on" in 1998, the year of the Vermont reform. If the Vermont reform were introducing significant bias into the estimates, the $\hat{\beta}_{placebo}$ coefficient would likely be precise.

The logic of the classic monocentric land use model (Alonso 1964, Mills 1967 and Muth 1969) and more recent models of urban growth based upon it (e.g. Arnott and Lewis 1979, Capozza and Helsley 1989, Wheaton 1982) suggests that the amount of land available for development will increase with distance from the urban center. Table 5 shows that this prediction holds in New Hampshire, as both housing unit density and the percent of land which is developed are higher in the area within 50 miles of Boston. This area will be termed the "suburban ring" and is mapped on Figure 4. Within New Hampshire, 50 miles is approximately the 10th percentile of distance from Boston and the closest New Hampshire community to Boston is located 33 miles from the city.

Land use regulation tends to originate in central cities and then spread outward to surrounding areas (Fischel 2004, Rudel 1989). Accordingly, regulation is likely most intense near the urban center. This hypothesis appears to hold in New Hampshire; the prevalence of a fairly stringent land use regulation – a form of growth management which permits municipalities to set a binding limit on the number of new homes built each year – is much higher within the suburban ring.

Figure 5 displays the full spatial distribution of both land availability and growth management. Figure 6 provides additional information on land use regulation by documenting that the stringency of zoning and the frequency with which subdivision regulations (a form of zoning) are updated are both strongly related to distance from Boston (see the notes on Figure 5 and 6 for an explanation and the Data Appendix for information on the zoning variables used on Figure 6).²²

The logic of the monocentric model, combined with the above stylized facts, motivate a specification which adds to equation (14) and an interaction between the fiscal shock and distance from Boston. The main effect interaction is also included (see the note to Table 6). Although the expectation is for the investment response to increase with distance from Boston because the supply elasticity should increase as you move away from the urban center, the specification, presented in column (1) of Table 6, suggests the opposite. To aid in interpreting the results, the rows at the bottom of the table present the implied change in the rate of investment, evaluated at the mean fiscal shock, for communities at the 25th, 50th and 75th percentiles of distance from Boston. Hypothesis tests for being able to distinguish these effects from zero are also presented.

It is possible that the effect of distance from the center city operates in a non-linear fashion. Column (2) therefore adds an interaction between the shock and an indicator for being located within the suburban ring. The estimate indicates the response to the reform was subject to

²²There is no relationship between the magnitude of the fiscal shock and distance from Boston. See Table 5. Furthermore, cross-sectional regressions (unreported) of the fiscal shock on either distance from Boston or a quadratic in distance from Boston produce imprecisely estimated coefficients.

significant geographic heterogeneity. The coefficient on the fiscal shock-suburban ring interaction term is of similar magnitude as the fiscal shock coefficient, but is of the opposite sign, whereas the fiscal shock-distance interaction coefficient is negative. Thus, there is no evidence that those communities nearest Boston experience a response to the fiscal shock. The communities just outside the ring, which could be thought of as exurban communities, experience the largest response and the impact of the reform then dissipates linearly with distance from Boston. Figure 7 provides a heuristic derivation of the effect. A community located at the 25th percentile of distance from Boston (60 miles), experienced a 0.64 increase in the rate of investment, equal to approximately four times the 0.17 effect for the entire sample (see column (1) of Table 2). A community located at the 75th percentile of distance from Boston (105 miles) experiences an increase in investment of 0.19. Columns (3) and (4) demonstrate that the results are robust to inclusion of the set of base-year interaction terms and, unlike the results on Table 2, municipal linear trend terms.

Figure 8 presents results from a more flexible specification which allows the impact of the reform to vary with a quartic in distance from Boston (see the note on Figure 8). The evidence again suggests that the response to the fiscal shock was quite concentrated in the area just outside the suburban ring. The treatment effect becomes distinguishable from zero around the 50 mile mark, rises quite sharply, peaks at around the 70 mile mark and then gradually declines, becoming indistinguishable from zero at around the 100 mile mark. These results, along with those on Table 6, suggest that land is in very inelastic supply in the area of the state nearest Boston, but that the elasticity rises extremely quickly as you move away from the urban center.

Figure 9 displays the geographic distribution of overall building activity. Residential investment is highest at the Massachusetts border and declines very gradually with distance from Boston. Even though the results of this paper suggest housing supply is relatively inelastic near Boston, total quantities are high in this area, likely reflecting intense demand for homes. Comparison of Figures 8 and 9 reveals that the building response to the fiscal shock is much more concentrated than is overall building activity.

It seems improbable that the supply elasticity falls as you move into the mainly rural portion of the state beyond the 70 mile mark, where there is ample land and only limited regulation. It is therefore likely that the decreasing investment response with distance from Boston starting around mile 70 is a demand-side phenomenon. As discussed in the theory section, when communities are close substitutes, a given fiscal shock will induce more homebuyers to switch communities than when the communities are imperfect substitutes. It is possible that the distribution of non-fiscal benefits varies systematically with distance from Boston. Individuals interested in purchasing a home in the exurban area just outside the fifty-mile ring may consider the communities to be close

substitutes. Individuals interested in purchasing homes in the relatively more rural portion of the state may consider the communities to be imperfect substitutes. For instance, homebuyers in rural areas may have strong personal attachments to given communities for reasons such as family ties and are less easily induced to switch communities in response to a fiscal shock. In terms of the model, π can be viewed as a positive function of distance d from Boston: $\frac{\partial \pi(d)}{\partial d} > 0$.²³ ²⁴

The geographic distribution of the investment response is consistent with Rudel (1989), who suggests the availability of land and associated lack of regulation eases the process of residential development at the urban fringe. Table 7 presents specifications which attempt to separately identify the impacts of land availability and land regulation on the investment response. Column (1) interacts the log of housing density with the fiscal shock (see the note to Table 7). The rows at the bottom present the implied change in investment induced by the grants at the 25th, 50th and 75th percentile of housing density and associated hypothesis tests. The results suggest that housing density has little impact on the marginal effect of the grants. With the inclusion of the distance to Boston-fiscal shock interaction term, to control for the influence of the π parameter, however, the marginal effect becomes significantly larger in low-density communities (column (2)).²⁵ As predicted, municipalities with more land remaining for development have a larger response to the fiscal shock. A similar result is found by Hilber and Mayer (2009).

Columns (3) and (4) replace the density measure with an indicator for having a growth management ordinance in 1999. Although there is no evidence that investment responds to the reform in communities with growth management (see the p-tests at the bottom of the table), the effect for these communities cannot be distinguished from the effect for communities without growth management. Additional specifications (unreported) fail to find evidence that elements of a town's zoning code as of 2000, including minimum lot size and required frontage, impact the investment response.

 $^{^{23}}$ Attempts (unreported) were made to proxy for π using information from the 2000 Census on average commuting times, percent of workers employed in their county of residence and percent of workers employed in New Hampshire. These variables potentially provide a gauge of the substitutability of communities (commuter communities may be more substitutable for one another than other types of communities). Inclusion of the fiscal shock term interacted with these proxies did not significantly alter the coefficients on the distance from Boston and suburban ring interactions, however. Their inclusion also had little impact on the capitalization results presented on Table 8 and discussed below.

²⁴An alternative possible explanation for the declining intensity with distance from Boston is that there is less building activity in the more remote portions of the state. With little activity, there would be little margin for a response to the fiscal shock. This may be a partial explanation. However, while investment does decline with distance from Boston, there is significant building activity throughout the state – see Figure 9. Even in the most distant municipalities there is a margin for response to the fiscal shock.

²⁵Use of the percent of land developed variable (displayed on Figure 5) in place of the housing density measure produces very similar, although slightly less precise, results.

Column (5) includes both the density and growth management interactions, as well as the suburban ring-fiscal shock and distance-fiscal shock interactions. The suburban ring interaction has a large and precise coefficient, whereas the density and growth management coefficients are imprecise, suggesting there is some unobserved characteristic of the suburban ring that limits its supply elasticity. Zoning codes and other land use regulations are often quite complex and hence difficult to quantify (Nechyba 2000). It is possible that the measures of regulation used here fail to fully chracterize the regulatory environment.

3.4.4 The Price Response: The Capitalization of the Fiscal Shock

If the supply of housing is not perfectly elastic, the fiscal shock will produce a price response. Column (1) of Table 8 presents the results of estimating equation (14) with the log of the mean sales price of existing homes as the dependent variable. The estimated response to the shock is positive, but is only marginally significant. Column (2) includes the suburban ring interaction term and demonstrates that the grants capitalize at a substantially higher rate in the area near Boston than in the rest of the state. Column (3) presents a falsification check (similar to the one in column (7) of Table 2). No evidence against a causal interpretation of the results in column (2) is provided. Columns (4) - (6) use a different measure of property values – the market value of all real estate, including commercial, as measured for tax purposes – and produces similar conclusions. Unreported specifications which include the distance to Boston interaction term produce similar results (the distance to Boston interaction term coefficient is imprecise).

The investment and capitalization results are complementary. It appears that the housing market cleared the fiscal shock primarily through a quantity response in most of the state. In the suburban ring, in contrast, the market cleared primarily through a price adjustment. These results are consistent with a substantially more inelastic supply of housing in the area near Boston. The claim that the market cleared the fiscal shock primarily through a price adjustment in the suburban ring implies the grants should have capitalized at close to their full discounted value in this area – i.e. the market should have moved from point X to point Z on Panel B of Figure 1. The estimates support this implication, as columns (2) and (5) imply seventy-five and ninety-five percent capitalization, respectively.²⁶

²⁶The full capitalization rate, $\frac{\Delta S_a}{k}$, is calculated with a discount rate k equal to 0.07, the 30-year conventional mortgage rate in 2000, and ΔS_a equal to the mean value of net grants in the suburban ring in the first-year of the reform. The actual capitalization rate is estimated using the value of all taxable municipal real estate in 1998, the year prior to the reform, and the estimates in columns (2) and (5).

3.4.5 The Regulatory Response

It is possible that the increase in residential construction sparked by the fiscal shock will cause voters to enact land use regulations aimed at reducing the pace of development or altering its form. Homeowners may wish to restrict new construction in order to mitigate costs associated with additional development and density, to prevent newcomers from free-riding on the existing tax base (Hamilton 1975, Hamilton 1976a), to extract rents from newcomers (White 1975) or to increase the value of their homes (Fischel 2001a).

Table 9 presents estimates of the impact of the fiscal shock on the probability of utilizing growth management and charging impact fees for development (these are the only regulatory measures avaliable at multiple points in time - see the Data Appendix for more information, including summary statistics). A two-period panel with data from 1999 and 2008 is used, both because of data limitations and because a regulatory response would be expected to manifest itself only gradually, after the increased pace of development becomes apparent.²⁷ The approach is otherwise the same as on the preceding tables. Column (1) shows that the fiscal shock is associated with an increased probability of utilizing growth management. Evaluated at the mean value of the fiscal shock, the estimate suggests that municipalities were 0.06 percentage points more likely to adopt the policy as a result of the reform – an increase of almost 50 percent relative to the probability of having adopted the policy as of 1999 (0.13). Column (2) documents that the results are robust to inclusion of the base characteristic-year interaction terms, an important finding given that the surge in housing demand over this period likely increased demand for regulation. The impetus to increase regulatory stringency is likely be the strongest in the areas which experience the largest increase in development. Column (3) therefore includes the suburban ring and distance from Boston interaction terms. Although the sign of the interaction coefficients are consistent with the area outside the suburban ring having experienced the largest increase in regulation, they are imprecisely estimated (the three fiscal shock coefficients are, though, jointly significant at the 5% level).

Columns (4), (5) and (6) present estimates for impact fees. No evidence is provided that the reform increased regulation on this margin. Impact fees may be used to address short-term concerns such as newcomers failing to cover one-time development costs (e.g. new sewer lines). Growth management, on the other hand, may be motivated by longer-run concerns such as preserving home values and avoiding excess density. If the desire for additional regulation was motivated by the longer-term consequences of elevated building activity, this may explain why the growth

²⁷The impact fee specifications use data from 2000 instead of 1999.

management results differ from the impact fee results.

The growth management results suggest that demand shocks cause land use regulation. They are therefore consistent with theoretical work suggesting relatively more dense and developed areas endogenously engage in more stringent land use regulation (Rudel 1989, Fischel 2001a, Hilber and Robert-Nicoud 2006 and Ortalo-Magné and Prat 2007). The results can also be seen as complementary to those in Saiz (2008) which document a similar causal relationship running from development and density to regulation, but over a much longer time horizon than examined here.

4 National Sample

This section explores the responsiveness of residential construction to changes in fiscal surplus using a national sample. While the use of a national sample does not permit the detailed analysis undertaken above, it is useful in establishing the generalizability of the New Hampshire results along three dimensions: geography, time – the data for the national sample are from an earlier period than the New Hampshire sample – and econometric technique – a cross-sectional IV strategy is used with the national sample, in contrast to the panel approach used with the New Hampshire data.

The empirical strategy and data used in this section were developed by Barrow and Rouse (2004), henceforth B&R, to examine the capitalization of school finance reforms.²⁸ The reduced-form estimating equation is

$$\frac{\Delta hstock_s}{hstock80_s} = \alpha + \beta \frac{newgrants_s}{pupils_s} + \varphi * X80_s + \varepsilon_s$$
(18)

where $hstock_s$ is the number of housing units in school district s, $hstock80_s$ is the number of housing units in a school district in 1980, $\frac{newgrants_s}{pupils_s}$ is the fiscal shock measure, the net new per-pupil state issued education grants received by a district in 1990 relative to 1980, $X80_s$ is a vector of school district and country characteristics measured in 1980 and the Δ operator takes the difference in the given variable from 1990 to 1980. Relative to the comparable New Hampshire specification, equation (14), $\Delta hstock_s$ replaces $permits_{m,t}$ (the variables are both measures of the number of new homes in a jurisdiction), the fiscal shock measure, the amount of new grants, is normalized by the number of pupils, as opposed to property tax revenue, and unified school districts are the unit of observation, as opposed to municipalities. These differences are dictated by data availability.

²⁸B&R find that, on average, the full present discounted value of grants issued as part of school finance reforms capitalize into residential property values.

Equation (18) is estimated on a single cross-section²⁹ using instrumental variables. The net new grants received between 1980 and 1990, $newgrants_s$, are almost certainly endogenous to determinants of residential investment. For instance, a school district with increasing poverty would experience an increase in grants in many states. If increasing poverty is associated with the propensity for investment, OLS estimates of β will be biased. In order to overcome this potential bias, $\frac{newgrants_s}{pupils_s}$ is instrumented with changes in state school financing formulas.

The instrument is constructed using state school financing formulas to predict the level of total state education aid in both 1980 and 1990. For both 1980 and 1990, the grant amount is predicted using the 1980 characteristics of the school district to ensure that variation in the instrument solely reflects changes in state aid formulas, not demographic changes at the school district level that may be associated with the level of residential investment. After the levels are constructed, they are differenced to obtain the instrument – the predicted change in state grants for education.

If the amount of residential investment during the 1980s is correlated with the 1980 characteristics of a school district, the IV results may be biased. In order to address this concern, the model is intensely saturated with a large vector of 1980 school district and county characteristics, $X80_s$. See the notes on Table 11 for a complete list of variables (the precise set of variables included in the $X80_s$ vector has little impact on the results).³⁰

The data comes from the 1980 and 1990 decennial census school district data files, the 1977 and 1987 Census of Governments, and the 1996 USA Counties CD-Rom. The instrumental variable is constructed using the state school finance formulas in the 1978 and 1990 Public School Finance Programs and data collected from individual states on tax rates and total assessed property valuations. See B&R for more information on the construction of the data.³¹ Table 10 presents summary statistics. The stock of housing increased by around 12 percent in the average school

²⁹ Equation (18) could also be viewed as a two-period panel with $\log(hstock_s)$ as the dependent variable and a school district fixed-effect differenced away. However, relative to the presentation of the New Hampshire specifications, which use the flow of new housing as the dependent variable, equation (18) should be viewed as a cross-sectional analysis.

 $^{^{30}}$ The IV specification follows that presented on Table 4 of B&R. Other than the different dependent variable, there are only three differences. First, the stock of housing is omitted from the $X80_s$ vector because the dependent variable is partially a function of this variable. Second, time-varying covariates are omitted out of concern that changes in community demographics may reflect endogenous adjustment to the policy change. For instance, inclusion of the change in pupils is problematic because a change in residential investment will mechanically cause a change in the number of pupils. Third, the geographic controls used differ – see the text for a discussion.

³¹The data is restricted to unified school districts which did not change boundaries between 1980 and 1990. School districts from the following states are dropped because they are not unified: Alaska, District of Columbia, Hawaii, Maryland, North Carolina and Virginia. Most of the districts in the following states are dropped because they are not unified: Connecticut, Massachusetts, Rhode Island and Tennessee. Districts which changed boundaries between 1980 and 1990 are dropped. Finally, California is dropped due to a lack of the data on property values required to construct the instrument. The sample includes 92 percent of independent unified school districts and 95 percent of all students enrolled in independent unified districts.

district over the course of the 1980s and the average school district experienced an increase in state grants of about \$1050 per pupil.

Column (1), Panel A of Table 11 presents OLS estimates of equation (18). The β estimates are negative and precisely estimated, suggesting that an increase in state education grants is associated with a large decrease in residential construction. These estimates are unlikely to represent the causal impact of state education aid on residential investment for the reasons discussed above.

Panel B presents the IV estimates. The first-stage estimates (unreported) suggest that a one dollar increase in predicted grants is associated with an increase in actual grants of approximately 10 cents, conditional on the covariates – quite similar to the first-stage coefficient reported in B&R. The first-stage is strong, with an F-statistics over 500. The second-stage estimates suggest that an increase in grants is associated with an increase in residential investment of $0.032.^{32}$ This effect is equal to around $\frac{1}{4}$ of the average residential investment rate during the 1980s. The results are sensitive to the inclusion of geographic controls. State fixed-effects substantially increase both the point estimate and the standard error (column (2)), while inclusion of census division fixed-effects produces estimates that are small and cannot be distinguished from zero (unreported).³³

Column (3) allows the response to vary by whether the school district is 100 percent urban or less than 100 percent urban (the purely urban districts account for 20 percent of enrollment in the sample). The results suggest that the response to the new grants is concentrated in districts which have at least some land classified as rural – i.e. areas where there is likely land available for development. B&R demonstrate that the degree of grant capitalization is strongly associated with demographic characteristics such as income and education. It is possible that the housing supply response is also influenced by these characteristics. In order to ensure that the results in column (3) do not reflect the impact of such demographic factors – the factors are highly correlated with the percent urban – the specification includes an interaction of the grant variable with an indicator for being in the top 20th percentile of income (this follows B&R– see their Table 6). Omitting the income interaction term has little impact on the results. Inclusion of additional characteristics shown to influence grant capitalization, such as education and the percent of housing units which are owner occupied, similarly has little impact on the results (unreported).

 $^{^{32}}$ The β coefficient has been rescaled by a factor of 1,000 (accomplished by multiplying the dependent variable by 1,000). The mean new grant per pupil is approximately 1,000 and the coefficient of .032 in column (1) can therefore be directly interpreted as the increase in the rate of residential construction for the district receiving the mean new grant.

³³Given that the policy changes used for identification occur at the state-level, it could be argued that state fixed-effects are the most appropriate geographic controls.

5 Policy Implications

The findings of this paper have important implications for reforms and policy initiatives which induce changes in property tax burdens and, more generally, fiscal surplus differentials across communities. The most significant example in recent years of this type of intervention has been school finance reform. Such reforms typically increase the importance of state financing at the expense of local sources of funding. To the extent that such actions produce changes in the relative fiscal surpluses of communities, they will distort the geographic location of housing capital.³⁴ Such distortions should be included in the cost-benefit analysis of school finance reform.

The paper also has important implications for our understanding of who bears the burden of property taxation. The finding that capital flees high tax jurisdictions validates a key element of the new view of the property tax. The result is an important finding given the lack of empirical evidence in support of the new view (Nechyba 2000). The findings should not, however, be interpreted as invalidating the benefit view. It is unlikely that either view is strictly correct and elements of both views may be simultaneously valid (Ladd 1998a, Oates 2000). A portion of the property tax may act as a user charge and a portion may act as a distortionary tax on capital (Kotlikoff and Summers 1987; Wildsain 1986) and the views may be of differing relevance in different settings. It is likely that the benefit view is most relevant in a suburban settings (Ladd 1998a), a hypothesis supported by the results of this paper. The response to the reform in the suburban area near Boston confirms closely to that predicted by the benefit view. The shock capitalized into property values and there was no evidence of capital reallocation.

6 Conclusion

This paper documents important interactions between the market for local public goods and the supply side of the housing market. The supply of new housing is quite responsive to fiscal amenities, but only at the edge of urban areas and in rural areas. As a result, policies which shift the relative fiscal position of communities will distort the geographic allocation of capital in these locations — a finding with particular relevance for property taxation and school finance. The paper also documents that fiscal amenities play a significant role in household location decisions. Thus, it provides direct empirical support for the operative mechanism of the canonical Tiebout theory. Most previous tests of the theory have been indirect in nature.

 34 It is well established that school finance reforms also influence the price side of the housing market – e.g. see the capitalization estimates in Hoxby 2001 and Hoxby and Kuziemko 2004.

The paper also shows that Tiebout type concerns influence the evolution of existing communities and the formation of relatively new communities. More broadly, the results can be seen as documenting the within MSA response to a housing demand shock. The quantity response to such a shock occurs outside of the urban center and, at least in New Hampshire, the response is intensely concentrated at the urban fringe – far more concentrated than overall building activity. Furthermore, the estimates suggest that a housing demand shock induces land use regulation, providing evidence of causality running from density and development to regulation.

Finally, the paper suggests that failure to consider the supply side of the housing market may lead researchers to misinterpret capitalization results. For instance, the finding of no capitalization in response to the fiscal shocks induced by the New Hampshire reform (see Table 8, column (4) and Lutz (2006)) might lead a researcher to conclude that the tax reductions were not valued by the marginal homebuyer. Particular care must be taken in interpreting heterogeneity in the extent of capitalization. Heterogeneity may reflect variation in the willingness to pay for an amenity or it may reflect differing conditions on the supply side of the housing market.

7 Data Appendix

The Stock of Single Family Homes in 1996

The stock of single family homes in 1996 (the first year of the sample), $hstock_m$, is constructed as follows. The stock of single family homes in 1990 is obtained from the 1990 Census. The 1990 stock is then increased by the number of building permits issues between 1990 and 1995. This 1996 stock number is then adjusted as follows. The 1990 stock is grown out by the number of building permits issued between 1990 and 1999 to construct a 2000 stock measure. The difference between the 2000 constructed stock measure and the 2000 stock measured obtained from the 2000 Census is taken as the estimated error in the growth procedure. The 1996 stock measure is then adjusted using the estimated error under the assumption that the error is apportioned equally to each year between 1990 and 2000.

Omitted Observations

The observation from the municipality of Seabrook is omitted from the estimation sample. Seabrook contains a nuclear power plant. The plant was successively devalued over the course of the 1990s. As a result, Seabrook lost close to \$800 million in property value, a situation which generates uncertainty concerning the data quality of the variables pertaining to property wealth and property taxes (specifically, there appears to be longitudinal inconsistency in how the contribution of the power plant to Seabrook's tax base and tax revenue is handled). This is a unique situation unrelated to the school finance reform. Two municipalities participating in inter-state school districts (both municipalities are in cooperatives with municipalities in Vermont) are omitted from the sample. These municipalities are dropped due to longitudinal inconsistency in the data.

New England Control Group Definitions

The Southern Maine control group is defined as all Maine communities within 175 miles of Boston – over 99 percent of the New Hampshire sample lives within this radius. The Western Massachusetts control group is defined as all Massachusetts communities greater than 33 miles from Boston – the New Hampshire community closest to Boston is located 33 miles from the city – and west of 71.3837 W. The Southeastern Maine control group is defined as communities east of 70.1970 W and within 175 miles of Boston. Central New England is defined as communities in Massachusetts south of 42.1497 N, west of 71.3837 W and located 33 miles or more from Boston and communities in Rhode Island and Connecticut north of 41.8645 N. For the construction of the control groups, distance from Boston is measured as the physical distance. In all other cases in the paper, distance from Boston is measured in terms of driving distance.

Measures of Land Availability

Two measures of land availability are used. The first measure is the number of houses per square meter of land from the 2000 Census. The second measure is the percent of land which has been developed. Land is considered developed if it is in use for residential, commercial, industrial or transportation purposes. Land is considered undevelopable, and therefore not included in the denominator of the measure, if it is classified as: open water, perennial ice or snow, barren, or wetlands. The data was produced by Hilber and Mayer (2009) and is based on the National Land Cover Data 1992. In some cases, the data cannot be mapped uniquely into a single municipality. In these cases, the data is mapped into an area comprised of two or more municipalities and these communities are all assigned the same value.

Land Use Regulation

Growth management is a stringent form of land use regulation which permits municipalities to limit the number of land use permits, including building permits for residential homes, issued annually (see Table 5 and Figure 5). The zoning measures displayed on Figure 6 are measured as of 2008. The zoning index indicates how many specific forms of zoning a municipality was using in 2008. The forms of zoning are: wetlands protection, wetlands buffering, shoreland, aquifer, historic, use permits and impact fees. Subdivision regulations concern the layout of lots and streets and the provision of city services such as sewers.

Only two forms of land use regulation are available at multiple points in time – growth management and impact fees. These variables are used in the regressions on Table 9. Impact fees are paid at the time of development and are intended to cover the cost of the public infrastructure associated with development. The impact fee data is available as of 2000 in a dataset on New Hampshire zoning collected by Richard England. The 2008 data comes from the New Hampshire Office of Energy and Planning. Impact fees are the only aspect of zoning which can be linked across the two sources / time periods. The growth management data is available for 1999 and 2008. The data comes partially from a survey conducted by the author and the remaining data is from the Office of Energy and Planning.

Appendix Table A1³⁵
Percent of Municipalities with Land Use Regulation

	1999/2000	2008
Growth Management	0.13	0.26
Impact Fees	0.13	0.44

³⁵Note. The sample is restricted to municipalities with 1200 or more residents in 2000. The first column contains data from 1999 for growth management and from 2000 for impact fees.

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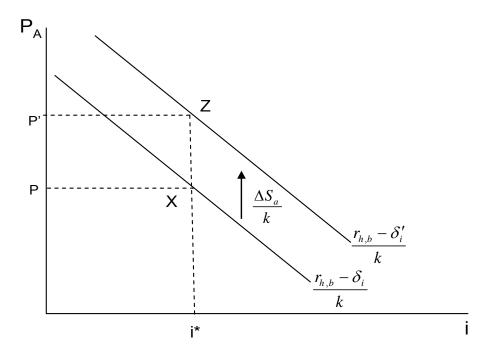
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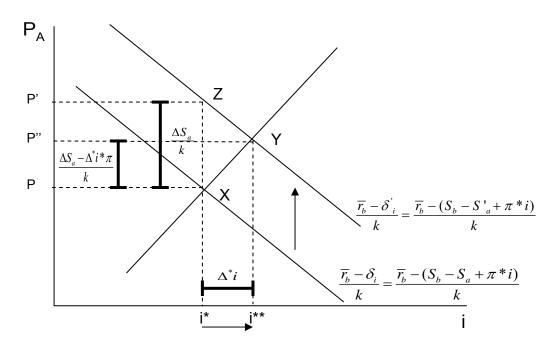
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Figure 1: Community A Housing Market

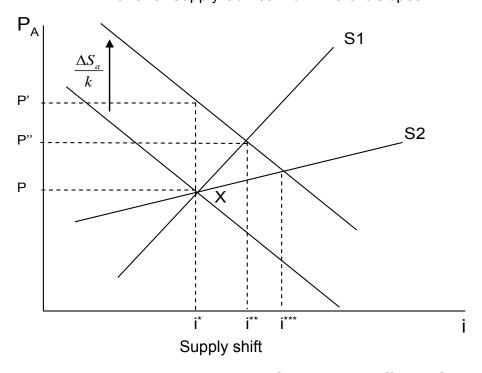
Panel A: Fiscal Shock with Fixed Housing Supply



Panel B: Fiscal Shock with Elastic Housing Supply



Panel C: Supply Curves with Different Slopes



Panel D: Aggregate Demand Curves with Different Slopes

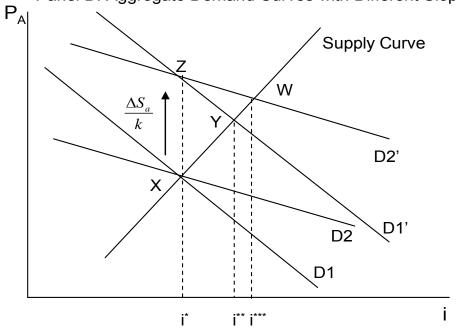
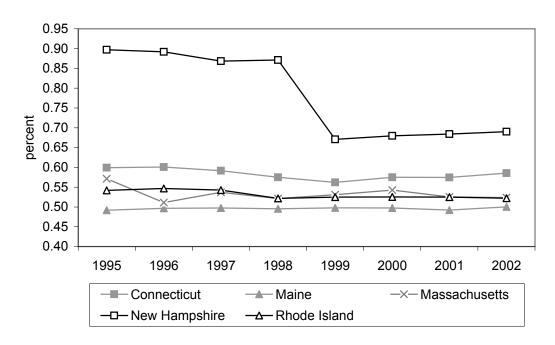
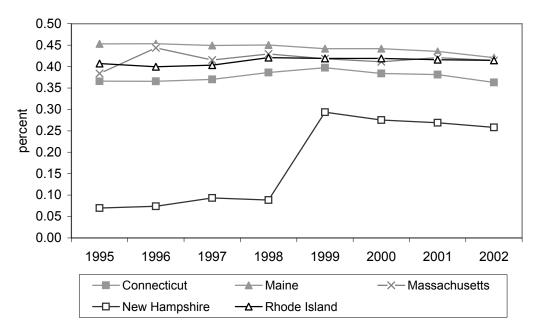


Figure 2: Education Funding by Level of Government

Panel A: Local Share of Education Funding

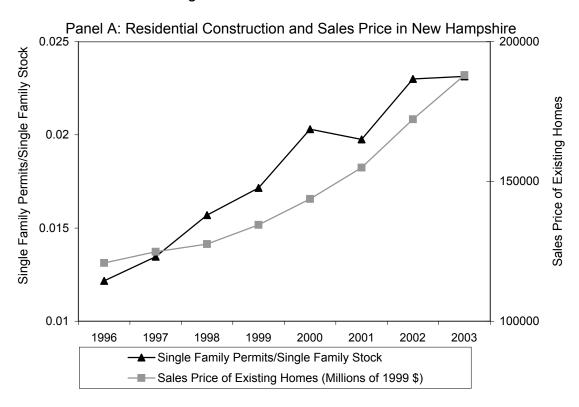


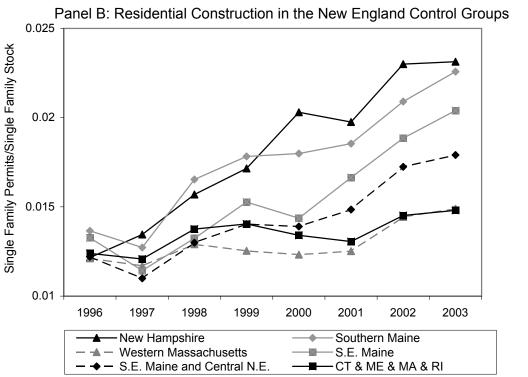
Panel B: State Share of Education Funding



Note. Data from Census Bureau School Finance data, New Hampshire Department of Education and New Hampshire Department of Revenue Administration.

Figure 3: Residential Construction



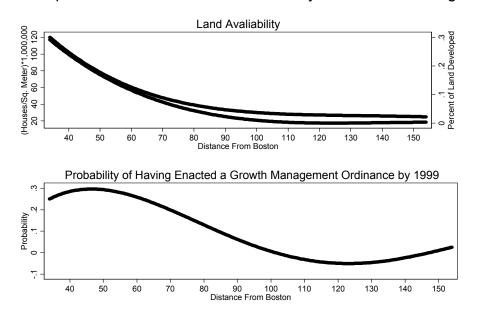


Note. The figures display municipality means for the sample of municipalities with at least 1200 residents in 2000 and which form a balanced panel over the period displayed. Building permit data from U.S. Census Bureau. Mean sales data in panel A are obtained from the New Hampshire Housing Finance Authority.

Southern Maine Southeastern Maine ■ Western Massachusetts Central New England New Hampshire Suburban Ring 200 miles 100 miles 50 miles Boston 33 miles

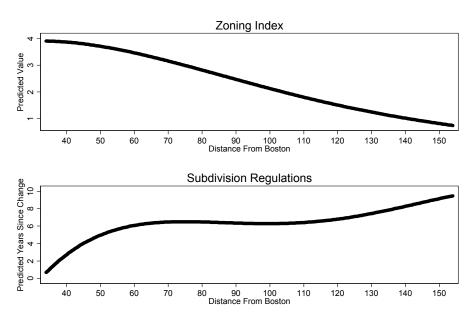
Figure 4: Monocentric Map of New England

Figure 5: Spatial Distribution of Land Availability and Growth Management



Note. The top panel displays the predicted total housing units (from the 2000 census) per square meter of land (left axis) and predicted percent of land in use (right axis). The bottom panel displays the predicted probability of having enacted a growth management ordinance as of 1999. The predicted values are produced by a cross-sectional OLS regression, with the muncipality as the unit of observation, of the given variable on a quartic in distance from Boston (in all three regression, the distance variables are jointly significant at the 1% level). The regressions are intended as a descriptive exercise only. The graphs end at the 95th percentile of distance from Boston (154 miles). See the Data Appendix for a discussion of the variables.

Figure 6: Spatial Distribution of Zoning in 2008



Note. The first panel displays the predicted value of the zoning index. The bottom panel displays the predicted number of years since a change in the municipal subdivision regulations. The predicted values are produced by a cross-sectional OLS regression of the given variable on a quartic in distance from Boston. The regressions are intended as a descriptive exercise only. The graphs end at the 95th percentils of distance from Boston (154 miles). See the Data Appendix for a discussion of the variables.

Figure 7: Geographic Heterogeneity in Investment Response

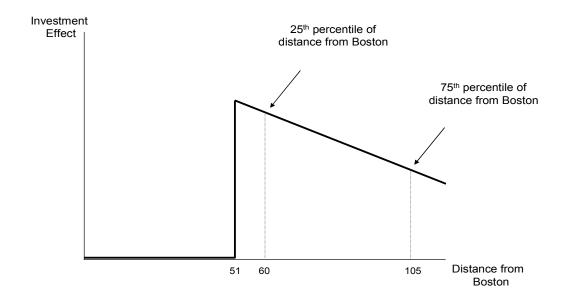
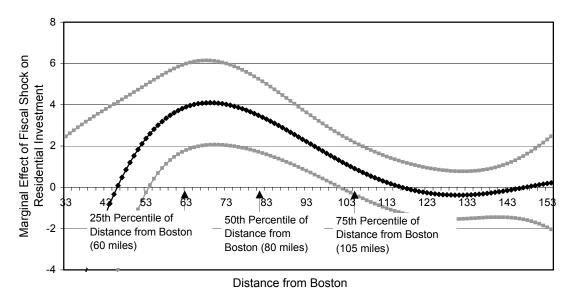
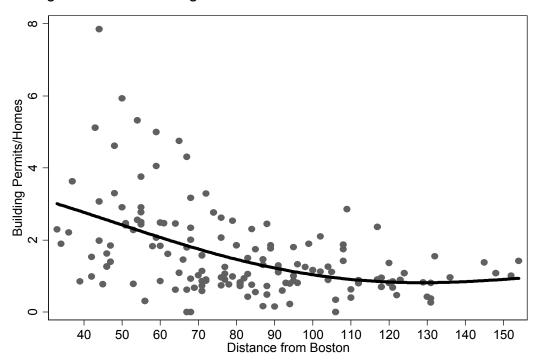


Figure 8: Fiscal Shock Interacted with Quartic in Distance from Boston



Note. The dark line displays the effect of the fiscal shock on residential investment as a function of distance from Boston. The light lines are 95% confidence intervals. The estimates are produced by a specification which includes the interaction of the fiscal shock variable with a quartic in distance from Boston and the interaction of a post-reform indicator variable with a quartic in distance, but is otherwise identical to specification used to produce the results in column (1) of Table 2. The figure displays the estimates up to the 95th percentile of distance from Boston (154 miles).

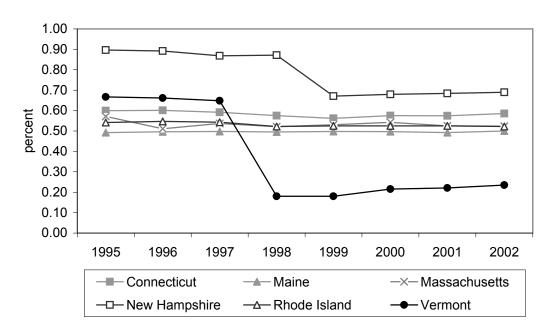
Figure 9: 1998 Building Rate Fit with Quartic in Distance From Boston



Note. The line is the fitted value from regressing the 1998 building rate on a quartic in distance from Boston. See the notes to Table 2 for information on the sample.

Appendix Figure A1

Panel A: Local Share of Education Funding



Panel B: State Share of Education Funding

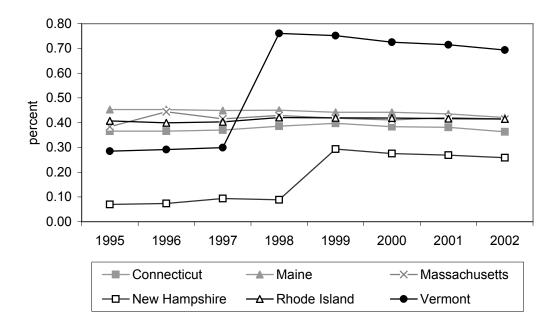


Table 1 Summary Statistics

row#		1998	2000	2002
1	1999 Net Education Grant / 1998 Local Property Tax Revenue	*	0.15	0.15
		*	(0.14)	(0.14)
2	10th Percentile	*	-0.05	-0.05
3	50th Percentile	*	0.17	0.17
4	90th Percentile	*	0.29	0.29
5	(1999 Net Education Grant / 1998 Taxable Property) * 1000	*	4.7	4.7
		*	(3.7)	(3.7)
6	Tax Rate Per \$1000 of Property	28.7	22.5	20.5
	(conditional on positive Net Reform Grant)	(5.4)	(4.5)	(4.8)
7	Tax Rate Per \$1000 of Property	15.1	15.6	13.5
	(conditional on negative Net Reform Grant)	(3.9)	(4.1)	(3.5)
8	Total Tax Payment (millions of 1999 dollars)	10.4	9.4	10.3
	(conditional on positive Net Reform Grant)	(16.6)	(15.0)	(15.9)
9	Total Tax Payment (millions of 1999 dollars)	8.5	9.7	10.7
	(conditional on negative Net Reform Grant)	(9.1)	(9.4)	(10.7)
10	Population	6977	7164	7392
		(11895)	(12141)	(12321)
11	Distance from Boston		86	
			(33)	
12	Number of Observations		158	

Note. The cells are municipality means unless stated otherwise. Standard deviations are in parentheses. The sample used to calculate the means is restricted to the set of districts with greater than 1200 residents in 2000 that form a balanced panel for the three years displayed. All variables are calculated with dollar values converted to 1999 dollars.

Table 2
Effect of Change in Fiscal Surplus on Residential Investment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Building	Building	Building	Log	Building	Building	Building
	Permits/	Permits/	Permits/	(Building	Permits/	Permits/	Permits/
	Homes	Homes	Homes	Permits)	Homes	Homes	Homes
(1999 Grant / 1998 Tax Revenue) * year >= 2000	1.15 (0.56)			0.98 (0.38)	2.41 (0.59)	0.44 (0.84)	1.19 (0.55)
(1999 Grant / 1998 Tax Revenue) * year = 2000		0.91 (0.51)					
(1999 Grant / 1998 Tax Revenue) * year = 2001		1.34 (0.61)					
(1999 Grant / 1998 Tax Revenue) * year = 2002		0.82 (0.66)					
(1999 Grant / 1998 Tax Revenue) * year = 2003		1.55 (0.82)					
(1999 Grant / 1998 Taxable Property) * year >= 2000			55.88 (20.89)				
(Placebo Grant / 1998 Tax Revenue) * year = (1998)							0.10 (0.30)
Implied Change in Dep. Var*	.17	*	.26	*	.35	.06	*
Implied Percent Change in Dep. Var*	.11	*	.17	.15	.22	.04	*
Number of Observations	1109	1109	1109	276	1109	1109	1109
Sample Collapsed to Single Pre & Post Observations Base Covariates * Year Indicators Municipal Linear Trends				X	X	X	

Note. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2003, with 1999 omitted from the sample (see text). All columns include a municipal fixed-effect and all columns other than (6) include year fixed-effects. Base covariates, interacted with year terms in columns (2), are distance from Boston, distance from Boston squared, municipal population, municipal population squared, the percent of municipal property that is residential, the percent of municipal residential property that is for seasonal or recreation use, and municipal density, defined as the total number of housing units divided by land area. * The implied change in the dependent variable and the implied percent change in the dependent variable are calculated using the mean value of the dependent variable in 1998 (the last pre-reform year) and mean value of the fiscal shock (1999 grant/1998 tax revenue) in 2000 (the first post-reform year). See text for a discussion of the placebo grant variable used in column (7). Tables displaying the complete set of coefficients available from the author upon request. For columns (1) - (5) and (7): The unit of observation is municipality-year. The dependent variable is the ratio of single family building permits to the number of single family homes in 1996 (the ratio has been multiplied by 100). The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. For column (6): The sample is summed to a single pre-reform observation and a single post-treatment observation for each municipality and the sample is limited to municipalities with a population exceeding 1600 in 2000 and which have non-missing building permit data for all years of the sample. The dependent variable is the log of the number of single family building permits. A single post indicator variable is included (to replace the year-effects).

Table 3
Demographic Characteristics of New England States

	Treatment Group		Control Groups					
	New Hampshire	Southern Maine	Western Massachusetts	Southeastern Maine	Southeastern Maine & Central New England	Connecticut Maine, Massachusetts, Rhode Island		
Per Child Residential Property Wealth	(1)	(2)	(3)	(4)	(5)	(6)		
	170,964	169,464	183,624	173,422	178,274	221,929		
	(83,297)	(107,041)	(75,098)	(78,270)	(73,792)	(137,583)		
Median Household Income	51,637	41,532	50,244	40,076	46,059	53,860		
	(12,795)	(7,942)	(11,846)	(06,043)	(11,504)	(19,425)		
Percent Beneath Poverty Line	0.06	0.09	0.07	0.09	0.08	0.07		
	(0.03)	(0.04)	(0.04)	(0.03)	(0.05)	(0.05)		
Percent Residents of School Age	0.27	0.26	0.26	0.25	0.26	0.26		
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)		
Median House Value	129,518	109,945	142,440	108,337	124,626	167,035		
	(42,527)	(32,518)	(37,161)	(28,693)	(34,897)	(94,782)		
Percent Non-White	0.03	0.02	0.05	0.02	0.04	0.06		
	(0.01)	(0.01)	(0.06)	(0.01)	(0.06)	(0.08)		
Unemployment Rate	0.04	0.05	0.04	0.05	0.04	0.05		
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)		
Percent of Houses for Recreation Use	0.12	0.16	0.06	0.16	0.11	0.08		
	(0.15)	(0.15)	(0.10)	(0.14)	(0.13)	(0.12)		
Single Family Homes	2,078	1,830	2,596	1,407	2,495	3,930		
	(2,443)	(1,660)	(3,520)	(1,046)	(3,305)	(4,256)		
Population	7,268	5,637	10,153	4,378	9,308	15,834		
	(12,165)	(7,386)	(17,233)	(5,325)	(16,628)	(29,824)		
Number of Observations	159	143	112	71	125	745		

Note. The cells display municipality means taken from the 2000 Census. Standard deviations in parentheses. The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. *See Figure 4 and the Data Appendix for precise definitions of the control groups.

Table 4
Effect of Change in Fiscal Surplus on Residential Investment: Robustness Check using Data from other New England States

		Control Groups									
	No Control Group	Souther	n Maine		stern chusetts	Southeast	ern Maine	& Cent	tern Maine ral New gland	Ma Massachi	ecticut, nine, usetts, and to Island
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
				В	uilding Pe	rmits / Hom	ies				
(Grant / Tax Revenue) * year >= 2000	0.45 (0.87)	1.10 (0.52)	1.53 (0.89)	2.26 (0.61)	1.89 (0.93)	1.31 (0.59)	1.96 (0.98)	1.54 (0.56)	1.87 (0.92)	2.30 (0.50)	1.57 (0.79)
Implied Change in Dep. Var* Implied Percent Change in Dep. Var* Number of Observations	.07 .04 1109	.16 .10 2108	.22 .14 2108	.33 .21 1892	.28 .18 1892	.19 .12 1605	.29 .18 1605	.22 .14 1983	.27 .17 1983	.34 .21 6309	.23 .15 6309
Grant Predictor * year >= 2000 Municipal Linear Trends	X	X	X X	X	X X	X	X X	X	X X	X	X X

Note. Grant / Tax Revenue refers to the ratio of the net grant in 1999 to property tax revenue in 1998. The unit of observation is municipality-year. The dependent variable is the ratio of single family building permits to the number of single family homes in 1996. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2003, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal and year fixed-effects and a control for per child residential housing wealth interacted with a post-reform indicator variable. See the Data Appendix and Figure 4 for precise definitions of the control groups. * The implied change in the dependent variable are calculated using the mean sample value of the dependent variable and the fiscal shock (grant / tax revenue).

Table 5
Summary Statistics for Within and Outside 50 Mile Suburban Ring

	In Suburban Ring	Outside Suburban Ring
1999 Net Education Grant / 1998 Local Property Tax Revenue	0.144	0.148
	(0.099)	(0.144)
(1998 Building Permits / Housing Stock) * 100	0.027	0.014
	(0.019)	(0.010)
Population	13353	6215
•	(18925)	(10520)
Total Housing Unit Density	84.6	34.8
	(99.3)	(56.0)
Percent of Land Developed	0.20	0.07
•	(0.160)	(0.098)
Growth Management Ordinance in 1999	0.33	0.10
	(0.48)	(0.30)
Number of Observations	21	137

Note. The cells are municipality means. Standard deviations are in parentheses. The sample is restricted to municipalities with greater than or equal to 1200 residents in 2000. Total housing unit density is defined as the number of total housing units in 1996 per square meter of land multiplied by 1,000,000.

Table 6
Effect of Change in Fiscal Surplus on Residential Investment: Heterogeneity by Distance from Boston

	(1)	(2)	(3)	(4)
(Grant / Tax Revenue) * (year >= 2000)	4.76 (1.92)	8.46 (1.97)	6.28 (2.06)	9.89 (3.39)
(Grant / Tax Revenue) * (year >= 2000) * (distance from Boston)	-0.04 (0.02)	-0.07 (0.02)	-0.03 (0.02)	-0.09 (0.03)
(Grant / Tax Revenue) * (year >= 2000) * (<= 50 miles from Boston)		-8.85 (3.02)	-8.55 (3.18)	-16.16 (6.49)
Implied Change in Dep. Var				
25th Percentile of Distance from Boston: 60 miles	0.38	0.64	0.62	0.69
50th Percentile of Distance from Boston: 80 miles	0.28	0.44	0.52	0.44
75th Percentile of Distance from Boston: 105 miles	0.15	0.19	0.40	0.12
P-value for test: Effect on Building 60 Miles from Boston = 0	0.01	0.00	0.00	0.01
P-value for test: Effect on Building 80 Miles from Boston = 0	0.01	0.00	0.00	0.01
P-value for test: Effect on Building 105 Miles from Boston = 0	0.02	0.00	0.00	0.27
Suburban Ring Hypothesis Testing (<= 50 miles from Boston)				
P-value for test: Effect on Building 33 Miles from Boston = 0	*	.34	0.25	.14
P-value for test: Effect on Building 50 Miles from Boston = 0	*	.17	0.17	.09
Number of Observations Base Covariates * Year Indicators	1109	1109	1109 X	1109
Municipal Linear Trends				X

Note. Grant / Tax Revenue refers to the ratio of the net grant in 1999 to property tax revenue in 1998. The unit of observation is municipality-year. The dependent variable is the ratio of single family building permits to the number of single family homes in the 2000 Census. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2003, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal and year fixed-effects. Column (3) include a set of time-invariant control variables interacted with a full set of year indicator variables. The variables are distance from Boston, distance from Boston squared, municipal population, municipal population squared, the percent of municipal property that is commercial, the percent of municipal residential property that is for seasonal or recreation use, and municipal density, defined as the total number of housing units divided by land area. Tables displaying the complete set of coefficients available from the author upon request. All columns include main interaction effects: For example, column (2) includes: (distance from Boston) * (year >= 2000) and (<= 50 miles from Boston)*(year >= 2000). The coefficient estimates for these main effects are not shown due to space limitations.

Table 7
Effect of Change in Fiscal Surplus on Residential Investment: Land Availability and Land Regulation

	Land Availability		Land Regulation		Land Availability and Regulation	
	(1)	(2)	(3)	(4)	(5)	
(Grant / Tax Revenue) * (year >= 2000)	1.41 (1.57)	9.95 (3.97)	1.17 (0.56)	6.30 (2.56)	12.04 (4.96)	
(Grant / Tax Revenue) * (Log of Housing Density) * (year >= 2000)	-0.12 (0.49)	-1.27 (0.62)			-0.95 (0.66)	
(Grant / Tax Revenue) * (Growth Management in 1999) * (year >= 2000)			-0.50 (1.85)	-3.01 (2.27)	1.38 (1.92)	
(Grant / Tax Revenue) * (distance from Boston) * (year >= 2000)		-0.05 (0.02)		-0.05 (0.02)	-0.08 (0.03)	
(Grant / Tax Revenue) * (<= 55 miles from Boston) * (year >= 2000)					-7.19 (2.94)	
Implied Change in Dep. Var						
25th Percentile of Housing Density	0.16	0.36	*	*	0.46	
50th Percentile of Housing Density	0.15	0.25	*	*	0.38	
75th Percentile of Housing Density	0.14	0.11	*	*	0.27	
P-value for test: Effect on Building at 25th Percentile of Housing Density = 0	0.05	0.01	*	*	0.01	
P-value for test: Effect on Building at 50th Percentile of Housing Density = 0	0.04	0.01	*	*	0.01	
P-value for test: Effect on Building at 75th Percentile of Housing Density = 0	0.13	0.20	*	*	0.02	
P-value for test: Effect on Building in Municipality w/ Growth Management			.71	.67	.46	
Number of Observations	1109	1109	1109	1109	1109	

Note. Grant / Tax Revenue refers to the ratio of the net grant in 1999 to property tax revenue in 1998. The unit of observation is municipality-year. The dependent variable is the ratio of single family building permits to the number of single family homes in the 2000 Census. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2003, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal and year fixed-effects. All columns include main interaction effects: For example, column (2) includes in the specification (distance from Boston) * (year >= 2000) and (Log Of Housing Density)*(year >= 2000). The coefficient estimates for these main effects are not shown due to space limitations. Full set of results available from author upon request. In columns (2), (4) and (5), the "implied change in the dep. var" and "p-values for test:" rows are calculated at the sample mean of distance from Boston. In column (5), the "p-value for test: Effect on Building in Municipality w/ Growth Management" row is calculated at the sample mean of housing density.

Table 8
Effect of Change in Fiscal Surplus on Property Values

	•	Mean Sales Existing Hor		Log(Market Value of Taxable Property)			
	(1)	(2)	(3)	(4)	(5)	(6)	
(Grant / Tax Revenue) * (year >= 2000)	0.13 (0.07)	0.12 (0.08)	0.15 (0.09)	0.09 (0.07)	0.07 (0.07)	0.06 (0.07)	
(Grant / Tax Revenue) * (<= 50 miles from Boston)*(year >= 2000)		0.21 (0.15)	0.23 (0.21)		0.37 (0.07)	0.40 (0.17)	
(Placebo Grant) * (year = 1998)			0.09 (0.11)			-0.03 (0.03)	
(Placebo Grant) * (<= 50 miles from Boston)*(year = 1998)			0.06 (0.43)			0.09 (0.05)	
Implied Percent Change in Property Values: <= 50 Miles fr. Boston		0.05	0.06		0.06	0.07	
P-value for test: Effect on Building <= 50 Miles from Boston		0.01	0.05		0.00	0.00	
Implied Placebo Percent Change in Property Values: <= 50 Miles fr. Boston			0.02			0.01	
P-value for test : Placebo Effect on Building <= 50 Miles from Boston			0.73			0.12	
Number of Observations	1113	1113	1113	1113	1113	1113	

Note. Grant / Tax Revenue refers to the ratio of the net grant in 1999 to property tax revenue in 1998. The unit of observation is municipality-year. The dependent variable is given in the column header. Standard errors clustered by municipality are in parentheses. The date range of the data is 1996 to 2003, with 1999 omitted from the sample (see text). The sample is restricted to the set of municipalities with greater than 1200 residents in 2000 with at least six years of non-missing building permit data. All columns include municipal and year fixed-effects. See text for a discussion of the placebo grant variable used in column (3). Columns (2),(3), (5), and (6) include main interaction effects: For example, column (2) includes in the specification (<= 55 miles from Boston)*(year >= 2000). Tables displaying the complete set of coefficients available from the author upon request. The implied percent change in property values is calculated using the mean sample value of the (grant/tax revenue) variable.

Table 9
Effect of Change in Fiscal Surplus on Land Use Regulation

	Growth Management			Impact Fees			
	(1)	(2)	(3)	(4)	(5)	(6)	
(Grant / Tax Revenue) * (year = 2008)	0.42 (0.14)	0.44 (0.19)	1.01 (0.56)	0.01 (0.26)	0.10 (0.32)	0.98 (0.99)	
(Grant / Tax Revenue) * (distance to Boston) * (year = 2008)			-0.007 (0.004)			-0.10 (0.01)	
(Grant / Tax Revenue) * (<= 50 miles from Boston) * (year = 2008)			-0.22 (0.53)			-0.86 (1.18)	
Implied Change in Dep. Var	0.06	0.06	*	0.00	0.02	*	
Number of Observations <u>Base Covariates * Year Indicators</u>	330	330 X	330	330	330 X	330	

Note. Grant / Tax Revenue refers to the ratio of the net grant in 1999 to property tax revenue in 1998. The unit of observation is municipality-year. All columns use a two-period panel. Columns (1) - (3) use data from years 1999 and 2008. Column (4) - (6) use data from years 2000 and 2008. The dependent variable is an indicator variable for the form of land use regulation given in the column header (and the specifications are therefore linear probability models). Standard errors clustered by municipality are in parentheses. The sample is restricted to the set of municipalities with greater than 1200 residents in 2000. All columns include municipal and year fixed-effects. Columns (3) and (5) include main interactions effects: (distance from Boston) * (year >= 2000) and (<= 50 miles from Boston)*(year >= 2000). Columns (2) and (5) include a set of time-invariant control variables interacted with a full set of year indicator variables. The variables are distance from Boston, distance from Boston squared, municipal population, municipal population squared, the percent of municipal property that is commercial, the percent of municipal residential property that is for seasonal or recreation use, and municipal density, defined as the total number of housing units divided by land area. Tables displaying the complete set of coefficients available from the author upon request. The implied change in the dependent variable is calculated using the mean value of the fiscal shock (1999 grant/1998 tax revenue) in 2000 (the first post-reform year).

Table 10 Summary Statistics : National Sample of School Districts

· · · · · · · · · · · · · · · · · · ·	
Δ Housing Units / Housing Units	0.12 (0.20)
100% Urban	0.03 (0.13)
< 100% Urban	0.15 (0.21)
Net New Grants Per Pupil (\$)	1054 (817)
Predicted Net New Grants Per Pupil (\$)	1147 (1719)
Total Housing Units, 1980	77620 (191585)
Number of Observations	9076

Note. Cells display means weighted by the number of 1980 pupils. Standard deviations are in parentheses. All variables are from 1990, unless otherwise stated. The data is from the sample of unified school districts constructed in Barrow and Rouse (2004).

Table 11
Effect of Change in Fiscal Surplus on Residential Investment: National Sample of School Districts

_	(1)	(2)	(3)		
	(Δ Housing Units / Housing Units) * 1000				
		A. OLS			
Net New Grants per Pupil	-0.047 (0.002)	-0.078 (0.003)			
Net New Grants per Pupil * (100% Urban)			-0.012 (0.004)		
Net New Grants per Pupil * (< 100% Urban)			-0.052 (0.003)		
Net New Grants per Pupil * (Top 20th Percentile of Average Household Income)			-0.032 (0.005)		
		B. IV			
Net New Grants per Pupil	0.032 (0.010)	0.118 (0.046)			
Net New Grants per Pupil * (100% Urban)			0.005 (0.013)		
Net New Grants per Pupil * (< 100% Urban)			0.042 (0.017)		
Net New Grants per Pupil * (Top 20th Percentile of Average Household Income)			0.014 (0.027)		
Number of Observations 1980 School District and County Characteristics State Controls	9076 X	9076 X X	9076 X		

Note. Panel A displays OLS estimates and panel B displays instrumental variable estimates. The dependent variable is (Δ Housing Units / Housing Units) * 1,000. Standard errors in parentheses. The sample contains unified school districts in 1990. All coumns are weighted by the number of pupils in 1980. In column2 (1) and (2) of panel B the instrument is predicted net new grants per pupil. In column (3) the instruments are the interaction of predicted net new grants per pupil with indicator variables for 100% urban, less than 100% urban and being in the top 20th percentile of average household income. All columns include indicator variables for the state in which the district is located and the following school district characteristics, measured as of 1980: unemployment rate, percent owner occupied housing, percent in housing built 10 or more years ago, percent moved into house less than 10 years ago, indicator for missing percent moved into house less than 10 years ago, number of vacant houses, percent attending private school, indicator for missing the percent attending private school, percent of population with 16 or more years of education, number of pupils, percent of school district which is urban. Column (2) also includes three indicators for 100% urban, less than 100% urban and being in the top 20th percentile of average household income. All columns include the following 1980 county characteristics: serious crime index, indicator for missing serious crime index, percent of Republican votes for president, percent of Democratic votes for president, percent of non-education employment organized, and the percent of employment in manufacturing.